Political Representation and Redistribution: The Economics of Enfranchisement and Undocumented Migration

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Foreword

The distribution of wealth and income, of opportunity and political power each have a long intellectual history. Since the Great Recession of 2008, the debate concerning these issues has intensified as scholars of all backgrounds strive to offer accounts of the causes and consequences of the unequal distribution of political and economic outcomes and opportunities. Economists participate in this debate and have provided fruitful analyses. On the one hand, scholars like Piketty (2014), Piketty and Zucman (2014) and Alvaredo et al. (2013) have shed light on the growing concentration of wealth at the top of the income distribution and have focused attention on the role that factors such as the returns to capital, the decline of top marginal tax rates and the decline in the bargaining power of labour play in explaining the dynamics of wealth and income inequality. On the other hand, scholars such as Acemoglu and Robinson (2015), Autor (2014) and Goldin and Katz (2009) offer explanations that centre more on factors such as skills and education, the endogenous evolution of technology and political institutions which do so much to shape how markets function and how their outcomes are distributed. In this respect, a number of economists have explored the dynamic interaction between political and economic forces in explaining inequality. Their work highlights the importance that such factors as the extension of suffrage, voter participation and political representation play in shaping political and economic outcomes (Cascio and Washington, 2014; Miller, 2008; Fujiwara, 2015; Pande, 2003). The overarching goal of this dissertation is to contribute to this literature by highlighting the economic and political impact of political participation. The question that unites the chapters in this volume is as follows: what happens to the distribution of political power and economic resources when ordinary citizens participate more fully in the affairs of society? Although the chapters are not theoretical in nature, the empirical analysis in each does rely on economic theory so as to shed light on the underlying mechanisms at work. In Chapter 1, participation is measured by rates of turnout in federal elections across the OECD countries while in Chapters 2 and 3, participation is measured through a natural experiment that legalised, and eventually enfranchised, a large number of Hispanic migrants in the United States in

the 1980s. The results in all three indicate that greater participation of people in the political process leads to significant increases in the distribution of economic resources as well as in political representation.

What follows is an overview of each of the chapters. Each chapter is self-contained and, as such, can be read independently. A consolidated bibliography is presented at the end of the work.

In Chapter 1, I test the predictions of the median voter theorem, which maintains that politicians, in an effort to stay in power, offer policy platforms that cater to the policy preferences of the person in the median of the voting distribution. I was motivated by the observation that turnout in major federal elections in many stable democracies has fallen drastically in recent decades. Because this decrease is strongly correlated with income, the pivotal voter is no longer the one whose income lies at the median of the overall income distribution but instead the one whose income lies at the median of a much richer subset of it. I therefore assemble a panel of the 34 OECD countries for the period between 1974 and 2014 in order to test the impact of voter turnout on the size of government, using top marginal tax rates as a key outcome variable. Across a number of specifications, I find that voter turnout does indeed have a positive and statistically significant effect on top tax rates. A limitation, however, of large, cross-sectional studies like this is that sharp identification, from which causal inference can be made, is difficult. Yet, the results of the paper seem to suggest more than just a spurious correlation. The results across a number of different specifications, including an instrumental variables strategy, all point in the same direction: that greater turnout has a positive effect on government redistributive policy. I conclude the paper by suggesting that the results of the study, far from casting doubt on the nature of the relationship, call out for further research, perhaps using microdata in order to benefit from sharper identification strategies. Chapters 2 and 3 are a response to that call.

In Chapter 2, which is joint work with Christoph Winter, we ask what happens to the distribution of public resources when undocumented migrants obtain legal status, and eventually citizenship and the right to vote, through a nation-wide amnesty. Our analysis is motivated by a model of distributive politics in which incumbent politicians respond to changes in the electorate by adjusting their budget allocations to target the preferences of the newly enfranchised group so as to optimise (a) the welfare of the population and (b) their own re-election chances. Accordingly, we posit that incumbent politicians will allocate resources strategically to those counties most affected by amnesty in an effort to win the political support of the newly legalised migrants. We test this model by exploiting the 1986 Immigration Reform

and Control Act (IRCA) in the United States. The IRCA legalised some 3 million undocumented migrants, mostly of Hispanic origin, and offered them a path to citizenship five years after legalisation. Utilising a difference-in-differences regression framework, we find that state governments allocate more per capita resources to those counties with a greater share of IRCA documented migrants. Probing further, we find that this allocation is sensitive to the political context of the state governor: counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition, enjoys line-item veto power or is politically aligned with the state legislature. Aggregating the variables of interest to the state level, we find that an incumbent governor's chances for re-election improve as the share of newly documented migrants in a state increases, further highlighting the political economy nature of our results. Given that undocumented migration is a politically and socially charged issue, we end the paper by trying to shed light on whether the allocation of resources to IRCA-affected counties is indeed intended to reach the newly documented migrants in an effort to win their vote or to satisfy other, perhaps competing, constituents. To this end, we exploit survey data as well as voting data on a key anti-migrant ballot measure and find no evidence that anti-migrant sentiment is confounding our results. Finally, we find that local expenditure in education increases in the share of newly documented migrants in a county. Hispanic individuals, as opposed to Caucasian ones, who reside in counties affected by the IRCA and who entered middle school after 1986 experience significant improvements in the likelihood of completing high school, suggesting that the additional resources were in fact intended to target the newly legalised migrants. On the whole, the results of the paper suggest that the relationship between legal status and the distribution of public resources is one of discretionary political choice rather than one of economic necessity or mechanical welfare increases.

Chapter 3 is, in many ways, a follow up to Chapter 2. Here, I test another potential channel through which Hispanic legalisation, and eventual enfranchisement, could have affected economic and political outcomes: political representation. In this case, the analysis is guided by models of political selection which posit that candidates of political office are selected primarily on account of their ability to enact policy. If policy competence, however, is concentrated among a certain group or class of people, then the electorate, according to these models, will opt for less able candidates in favour of greater representation. Although some empirical work has shown that the competence-representation duality does not always exist, when the interests of groups are far apart or when competence is unevenly distributed, it is not clear to what extent competence or representation will drive political selection. In a setting such as

the United States in 1986, the three million Hispanic migrants affected by the IRCA systematically differ in their socio-economic characteristics to the rest of the population. Accordingly, I hypothesize that their legalisation and eventual enfranchisement will lead to increases in the number of Hispanic individuals elected to public office and it will do so on account of the ability of these officials to better represent Hispanic interests. To test this potential explanation, I digitised a novel source of data that contains information on Hispanic persons elected to public office in the United States from the local to the federal level from 1984 to 1994 and compare the number of such officials in counties more affected by the IRCA with those in counties less affected by the IRCA both before and after 1986. I find that counties with a greater share of newly documented migrants also experienced significant increases in the number of Hispanic persons holding political office who also reside in that county. The timing of the effect suggests that it is indeed the voting potential of the IRCA migrants that is driving the result: the number of Hispanic elected officials at all levels—local, state and federal—increases in counties affected by the IRCA only as of 1992, the first year when the newly documented migrants could vote. Decomposing the data further reveals that the result is driven almost entirely by stronger representation at the local level, in particular by members of local school boards and by mayors of small cities, offices where, arguably, community representation matters more than policy-making expertise. By contrast, I find no effect whatsoever for Hispanic elected judges at either the state or local level. These last two findings lead me to conclude that the Hispanics that are selected to public office as a result of the IRCA are selected not strictly on account of their educational qualifications and professional credentials but rather for their ability to better represent Hispanic interests on local matters pertaining to schooling and education.

Chapter 1

Turning Out for Redistribution: The Effect of Voter Turnout on Top Marginal Tax Rates

1.1. Introduction

On a single day in May 2012, some 100,000 people gathered in New York City[1] to protest the exorbitant rise in the share of incomes accruing to the top 1 percent of the income distribution in the United States. In that country, the pre-tax income share of the wealthiest 1 percent has increased from 11 percent in 1978 to some 20 percent in 2015 even as the income share of the bottom 50 percent has collapsed from 20 to 12 percent over the same time period (Alvaredo et al., 2017). Around the world, trends are similar. As of 2014, the richest percentile of the global population claimed 48 percent of the world's wealth (Oxfam, 2015). As such, the 'May Day' protest in New York City was organised as part of the larger *Occupy* movement which "aims to fight back against the richest 1 percent of people that are writing the rules of an unfair global economy."[2] The movement spanned some 100 cities in the United States and approximately 1,500 others across the globe. And yet, in November of that same year, just 53.5 percent of the total voting age population turned out to vote in America's presidential election. Ironically, individuals with incomes over US$100,000 comprised more than 25 percent of the vote.[3] More generally, across all OECD countries, those that held elections between 2010 and 2014 experienced an average turnout of 66 per-

[1] Estimates taken from the David Graeber's 7 May 2012 article in the Guardian: *http://bit.ly/2ttRH7L*. Accessed September 2016.

[2] Taken from the Website of the Occupy Wall Street: http://occupywallst.org/about/. Accessed September 2016.

[3] Authors own calculations using Table 7 of the U.S. Census Bureau, Current Population Survey, November 2012. Those with incomes of more than $100,000 per annum make up 26.5 percent of the registered voting population.

cent, 12 percentage points lower than the 1974–1979 average of 78 percent.[4] Although this evidence need not be contradictory—it could be, for example, that the protesters were mostly well-to-do voters whereas low income individuals neither voted nor protested—the contrasts are stark and they stimulate questions worthy study: for instance, if so many are concerned about the distribution of wealth and if so many are committed to taking action for the cause, then why do so few people vote? And what are the economic consequences of such low rates of turnout? Are they, in any way, responsible for the growing concentration of wealth among the top percentile?

In this chapter, I analyse the latter two of these questions. I examine the impact of voter turnout on top marginal tax rates in the Organisation for Economic Cooperation and Development (OECD) countries. Empirical studies have shown that those who vote are systematically different in their social and economic characteristics—and hence their policy preferences—to those who do not vote. A large body of scholarship indicates that, among other things, those who vote tend to be richer, better educated and older than those who do not. Falling rates of voter turnout, therefore, imply that the preferences of wealthy individuals are over-represented in relation to those of the population in general which, I expect, will place less pressure on public policy for redistribution. Accordingly, I anticipate top marginal tax rates to rise and fall with turnout.

To test this hypothesis, I assemble a panel of data for the OECD countries for the period between 1974 and 2014 to assess the impact of voter turnout in national elections on top marginal tax rates. I find a positive and statistically significant relationship between the two. Results across a number of specifications indicate that a ten percentage point increase in voter turnout leads to approximately 2.5 percentage point increase in the top marginal tax rate. If the correlations uncovered in this chapter are causal, then the 12 percentage point decrease in voter turnout in the 40 years to 2014 can explain some 13 percent of the 23 percentage point decline in tax rates over the same period. Because both variables are downward-sloping time series, I undertake a number of empirical strategies to regress them against each other in order to determine whether their relationship is, in fact, causal.

As a first step, I control for a number of observable social and economic characteristics of the countries in question, including per capita gross domestic product, annual growth in GDP, educational attainment, whether the election is presidential or parliamentary and the population size of a given country in a given year. To address concerns that deeper, underlying characteristics such as political freedoms are driving

[4]Turnout expressed as a fraction of the total voting age population (VAP) The difference between turnout and VAP turnout will be discussed later in the paper.

the results, I include indices from Freedom House that measure the degree of political and civil rights. Controlling for these observable characteristics does little to diminish the effect of voter turnout on top marginal tax rates. The results remain positive and statistically significant at conventional levels.

As a second strategy, I exploit the panel structure of the data by including year dummies and country fixed effects. This is a crucial step as it enables me to (a) study the within-country variation of the variables of interest and (b) account for all unobservable, time-invariant country heterogeneity. It also allows me to control for any additional unobservable effects that vary over time but that remain constant across countries such as shocks to the global economy. Encouragingly, I find that my results remain robust to the inclusion of both year dummies and country fixed effects.

To increase confidence in the OLS estimates, I re-run the regressions on various subsamples. First, I regress the top marginal tax rate in years $t+1$, $t+2$ and $t+3$ as well as over the election cycle on voter turnout for elections in year t and I find significant results, implying that turnout has an impact not just in the election year but throughout the entire term of a given government. Second, I carry out a placebo test by regressing tax rates in years $t-1$, $t-2$ and $t-3$ on voter turnout for elections in year t. If turnout is what causes changes in tax rates, then we would not expect to see significant effects of turnout in year t on tax rates in years $t-n$. Consistent with this thinking, I find that turnout has little to no explanatory power over top marginal tax rates in the years leading up to an election.

As a final step, I employ an instrumental variables approach to exploit only the exogenous variation in voter turnout. The instrument I use is an indicator variable for whether a country has compulsory voting laws, arguing that the introduction of such laws influences the top tax rate only and only through its impact on rates of turnout. The instrumental variables results produce estimates with coefficients of similar magnitude and sign to OLS estimates. However, coefficients are estimated less imprecisely than OLS and as such, are taken as suggestive.

This study contributes to a rich literature that documents the effect of voter turnout on economic outcomes. The most well-documented relationship between turnout and redistribution concerns the effect of voter participation on government expenditures as a share of GDP. Hicks and Swank (1992) examine the political determinants of social spending in 18 industrialised democracies during the period between 1960 and 1982 and find that higher rates of voter turnout augment welfare spending. In a similar vein, Lindert (1996) analyses the factors that determine social spending in a panel of data for 19 OECD countries between 1960 and 1981. His dependent variables include six measures of government expenditure: pensions, welfare, unemploy-

ment compensation, education, health and non-social spending. His results indicate that higher levels of voter turnout are associated with higher levels of social spending in almost all categories. Examining similar questions with another panel of 19 OECD countries, Franzese (2002) finds that both income skew and voter participation affect government transfers. Interestingly, his study also finds that the interaction of these two variables has a positive and significant effect on taxes and transfers, suggesting that voter turnout matters more for government redistribution when income distribution is more unequal. More recently, Mahler, Jesuit and Paradowski (2014) assess the impact of voter turnout across 14 advanced democracies. In contrast to other studies, the authors use a measure for turnout inequality rather than rates of turnout as the main explanatory variable and find that greater inequality of voting is associated with less social benefits. Larcinese (2007) carries out a cross-country panel analysis of 41 countries between 1972 and 1998 and finds a similar relationship between voter turnout and social and welfare spending. His contribution shows that the results derived in previous studies hold even when considering countries that are not "developed democracies"; in so doing he expands the sample and extends previously documented results to a much wider range of countries. Finally, Fujiwara (2015) examines the introduction of electronic voting technology in Brasil. He finds that such technology led to a large increase in voting among lesser educated citizens, thereby causing a shift in government spending towards health care aimed at lower income groups.

The study adds to this literature in two ways. First, it adds to studies like Lindert (1996), Franzese (2002) and Larcinese (2007) by analysing recent data for the OECD nations over a 40 year period. In this respect, Lindert (1996) explains that "post-1981 data are not available in the abundance and quality of this special OECD study". The dataset compiled for this study does not suffer from this difficulty. The number of countries in the OECD has also grown to 34 since Lindert's study which expands the sample considerably. Additionally, because of the panel structure of my data, I am able to account for any unobserved time and country heterogeneity which might confound the analysis, a step which has not always been taken in previous work. I also examine the effects of turnout on tax rates not just in the year of the election but in subsequent years and over the electoral cycle and I employ an instrumental variables strategy in an effort to isolate the exogenous variation in turnout.

Second, by investigating the effects of voter turnout on top marginal tax rates, the study is, to my knowledge, the first of its kind to measure the effects of voter participation on policies that *directly* affect the distribution of income. Thus, while it is related to the literature that examines the relationship between turnout and redistri-

bution as measured by the amount of government social spending, the study poses a slightly different question, asking instead what the direct effects of voter turnout are on the tax rates of the wealthiest segments of the population. In so doing, the study relates to the literature that documents the determinants of wealth concentration, the like of which include the difference between the interest rate and growth rate (Piketty, 2014; Piketty and Zucman, 2014), the supply and demand of skills and the overall returns to education (Autor, 2014) and institutional and technological factors (Acemoglu and Robinson, 2015). Although political accounts of inequality exist, these studies focus mostly on the power and resources of the wealthy in shaping market outcomes and government public policy (Hacker and Pierson 2010; Winters 2011). This study thus adds to this scholarship by relating political participation of citizens in elections with one tax policy intended to respond to the growing concentration of wealth.

The rest of this chapter is organised as follows. Section 1.2 discusses the median voter theorem and addresses the dual questions of who votes and why. Section 1.3 describes the data and also details the empirical specifications and results. Section 1.4 discusses the results and Section 1.5 concludes.

1.2. Theoretical Motivation

1.2.1 Voter Preferences and the Median Voter Theorem

A tenet of the political science of elections is the median voter theorem. The theorem maintains that politicians have but one objective: to win office. Accordingly, politicians have powerful incentives to align their policy platforms to suit the interests of the majority of their respective electorates. Consequently, the policy platforms of competing parties on a left-right policy spectrum gradually converge toward centre ground so as to maximize the number of votes won and to minimize the number of votes lost. As early as 1929, Harold Hotelling, observing the competition between the Republican and Democratic parties in the United States, noted that in order to avoid losing votes, "each party strives to make its platform as much like the other as possible" (Hotelling, 1929).

Elaborating the idea, Downs (1957) suggested that politicians "act solely in order to attain the income, prestige and power which come from being in office..." and that "parties formulate policies in order to win elections, rather than win elections in order to formulate policies". To win office, therefore, politicians offer policy platforms that increasingly draw nearer to the preferences of the voter whose preferences are located in the median of the preference distribution. Put more precisely, the Downsian

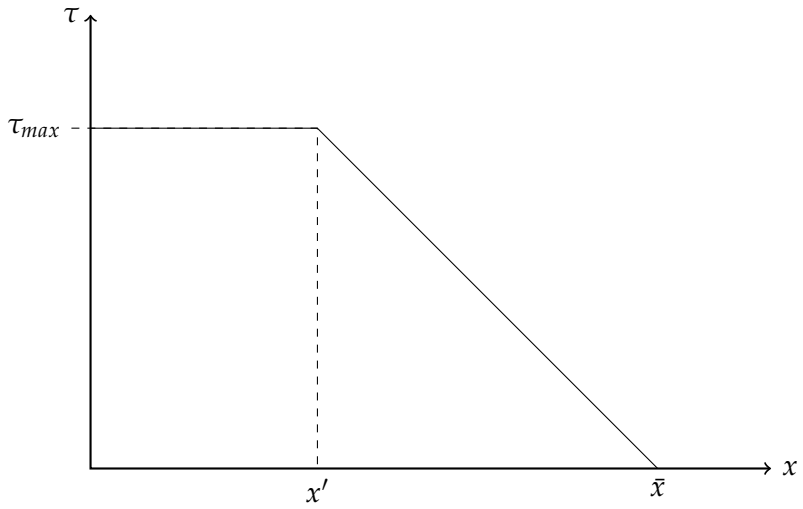

Figure 1.1: Tax rates as a function of individual income and productivity

model of electoral competition states that, given a one-dimensional policy space and single-peaked voter preferences, the policy preferences of the median voter act as a sort of centripetal force, drawing the policy platforms of various parties towards it.

Meltzer and Richard (1981) build on the Downsian conception of electoral competition to model how governments set tax rates and allocate social spending in response to the preferences of the median voter. Their seminal model indicates that the size of government, taken to be the share of income redistributed by the government in welfare payments and in social services, depends on the relative distance between median and mean income. The lower median income is with respect to mean income, the greater is the demand for redistribution, up to a maximum tax rate. As median income approaches mean income, the preferences of voters for a high tax rate declines. Politicians maximize their electoral chances by offering a tax rate commensurate with the preferences of the median voter for redistribution. The insights of their model are captured in Figure 1.1, where t denotes the tax rate and x denotes individual productivity endowments.

In their model, individual income, y, is a function of individual productivity, x. Tax rates are denoted by τ. Utility is a function of consumption and leisure and consumption, in turn, is a function of net income plus a government benefit, r, as shown in equation 1.1.

$$c(x) = (1 - \tau)xn + r \tag{1.1}$$

Where n is time allocated to work and $1 - n$ is time allocated for leisure. An

individual in a Meltzer and Richard economic environment thus tries to choose n so as to solve the following utility maximization problem:

$$\max_{n\epsilon[0,1]} \quad u(c,l) = u[r + nx(1-\tau), 1-n] \tag{1.2}$$

Without specifying a functional form for $u(c,l)$, Meltzer and Richard derive the following first-order condition, comprised of utility from consumption and utility from leisure.

$$\frac{\partial u}{\partial n} = u_c[r + nx(1-\tau), 1-n]x(1-\tau) - u_l[r + nx(1-\tau), 1-n] \tag{1.3}$$

The model suggests that there exists a certain level of productivity, denoted x_0, at which individuals subsist solely on government transfers and choose not to work. Setting $n = 0$ and solving for x in equation 1.3, we derive the threshold level of productivity below which individuals subsist on government benefits:

$$x_0 = \frac{u_c[r,1]}{u_l[r,1](1-\tau)} \tag{1.4}$$

Note that x_0 is maximized as τ increases. Consequently, individuals endowed with productivity $x \leq x_0$, prefer rates of taxation that maximize government transfers and, hence, individual utility (since utility is a function of government transfers, r, and productivity, x). If the median voter is endowed with x_0, tax rates are set at a maximum, noted τ_{max} in Figure 1.1. The maximum tax rate has an upper bound, beyond which overall output and, hence, government revenues decline. As the median income receiver increases in productivity, his or her preference for government transfers decreases as does his or her preference for a high tax rate. When the productivity of the decisive voter approaches average levels of productivity, $\bar{x}$, his or her preference for taxation go to zero as he or she derives full utility from income generated through work and time dedicated to leisure. Because utility is a function of both earned income *and* government benefits, votes for the tax rate are implicit votes for utility: people less reliant on government benefits prefer lower taxes whereas those more reliant on them prefer higher taxes. In a majority rule voting system, therefore, the pivotal voter is the one located at the median of the income distribution.

A more general implication of the model is that the more widely political franchise is extended, the poorer is the median voter in relation to the population average and the greater are his or her demands for redistribution. The model thus has trouble explaining rising inequality, especially of the sort generated by the hyper-concentration of wealth by the top percentiles. After all, such concentration creates median voters with incomes far below the average and with preferences for greater

redistribution. If the model holds true, then, we would expect tax rates to rise as wealth becomes increasingly concentrated. Why haven't they?

1.2.2 Who Votes?

The shortcomings of the median voter theorem have been well documented elsewhere (see, for example, Alesina (1988), Lee, Moretti and Butler (2004), or chapter 2 in Hindmor (2006)). Here, I focus on the role of turnout. The theorem assumes that everyone in a population votes, in which case the decisive voter is the voter with median level income as suggested by Meltzer and Richard. But what happens when not everyone votes? If voters and non-voters are randomly distributed, then it would be difficult to identify significant consequences of falling rates of turnout. As it happens, however, those who vote are typically better educated, wealthier and more informed politically than those who abstain. From an empirical point of view, then, the 'median voter' is not the one who lies at the median of the income distribution of the population but rather at the median of a much richer subset of the income distribution. Although there is some research that challenges the conventional view that voter turnout—and in particular the decline of it—is marked along class lines (Leighley and Nagler, 1992), the majority of studies consistently show that factors such as age, income, education as well as being better informed are strong predictors of individual turnout (Lassen 2005; Smets and van Ham 2013; Gallego 2010). Larcinese (2007) describes the phenomenon aptly:

> Downsian models normally assume that everybody votes... It is, however, clear from available data that non-voters are not randomly distributed across the total population: a substantial body of empirical research has documented that voters and non-voters systematically differ in their socio-economic and demographic background and, therefore, in their needs and policy preferences... In different countries and elections, empirical research consistently shows that the likelihood of voting is positively correlated with income, age and education level, as well as with being a male citizen. It is quite likely that such characteristics are correlated with policy preference, especially over issues of redistribution.

A cursory search of readily available evidence confirms this view. Figure 1.2 shows voter turnout by household income for the last three presidential elections in the United States. As shown, there is a clear association between income and turnout. Figure 1.3 shows the same data, this time for the 2014 presidential election and broken

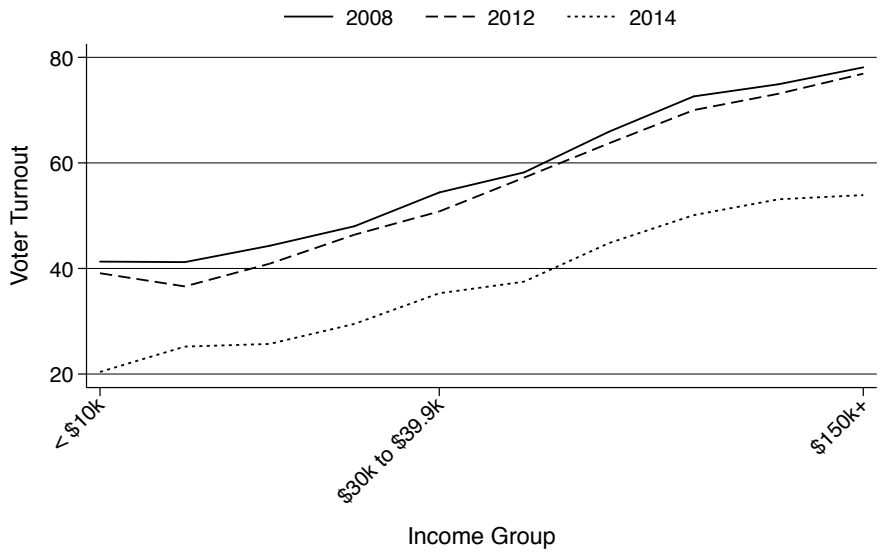

Figure 1.2: Turnout by household income in US presidential elections

Source: US Census Bureau Current Population Survey (Tables 7 for 2012 and 2014 and Table 8 for 2008)

Table 1.1: Turnout by income group in 1995 French presidential elections

	Turnout (%) by Income Group						
	Lowest	Low	Middle	High	Highest	Total	N
Did not vote	17.6	16.3	9.6	8.4	7.9	11.1	101
Voted	82.4	83.7	90.4	91.6	92.4	88.9	810
N	102	106	230	274	139	911	

Source: Abrial, Cautres and Mandran (2003)

down by age. Data for the 2008 and 2012 elections are similar: all show a positive correlation between age, income and voting. And such relationships are not just observed in the United States. Table 1.1 shows that voter turnout in France is also marked along income groups, with wealthier segments of the population displaying greater rates of electoral participation (Abrial, Cautres and Mandran, 2003).

1.2.3 Why Do People Vote?

How do we know that the direction of causation is from turnout to redistributive policy and not the other way around? That is to say, how do we know that more inequality and less redistributive policy themselves do not cause lower levels of turnout?

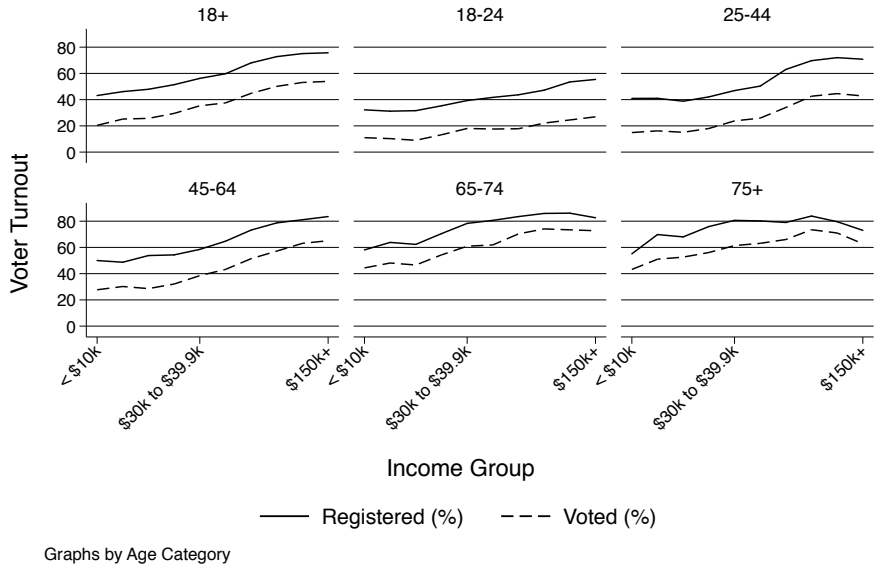

Figure 1.3: Turnout by household income and age in the 2014 US presidential election

Source: US Census Bureau, Table 7 of the November 2014 Current Population Survey

Whilst there may be a possibility of reverse causation, such a hypothesis has received little intellectual support. For one, all the theoretical implications of the median voter theorem suggest that the direction of causation is from voters to government. For another, the reasons why people vote are many but income distribution and taxation policy do not appear to be among them. Instead, theoretical and empirical work tends to point to such factors as the costs of registration (things like voter registration laws, information acquisition and time); demographic factors (including education, income, age, gender and race as discussed in the previous subsection); social and psychological factors (the like of which include habit, marital status, civic duty and social image); and the particular characteristics of a given election (tightness of the race) (Harder and Krosnick 2008; Stefano DellaVigna 2017).

Elections Canada commissioned a survey following its 2 May 2011 General Election in order to better understand the reasons why eligible voters did not cast their ballots. As shown in Figure 1.4, inequality, taxation policy or government redistribution do not appear as reasons for abstaining. Whilst these figures cannot conclusively rule out the possibility of reverse causality, the weight of evidence available seems to support the hypothesis put forward in this chapter.

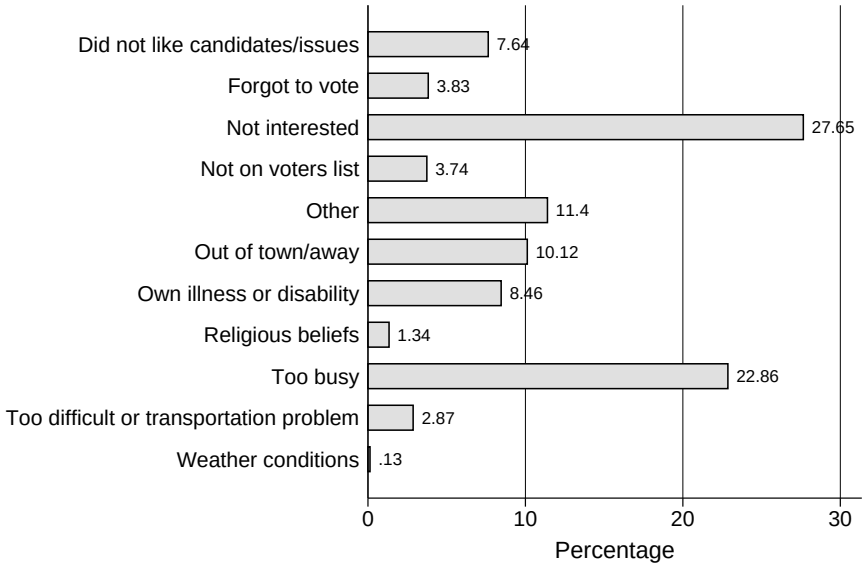

Figure 1.4: Why voters did not vote in Canada's 2011 General Election

Source: Labour Force Survey commissioned by Elections Canada, May 2011

1.3. Empirical Methodology

1.3.1 Data

The analysis is based on measures of voter turnout and top marginal tax rates in the 34 nations that comprise the Organisation for Economic Cooperation and Development (OECD) for the period between 1974 and 2014. The OECD countries were chosen for two reasons. First, by and large, the OECD Member States represent the advanced democracies of the world where voting and elections take place most regularly and it is the economic impact of voter turnout in such countries that we are interested to study. Second, and relatedly, data are much more readily available for the variables of interest within the universe of OECD nations. In this respect, a new dataset was compiled from a number of sources described in this section.

Data for voter turnout comes from the International Institute for Democratic and Electoral Assistance (IDEA). IDEA keeps two records for voter turnout: one, *turnout*, that measures voter participation as a percentage of registered voters and a second, *VAP turnout*, that measures voter participation as a fraction of the total voting age population. The fact that VAP turnout captures turnout as a fraction of the voting age population and not the *eligible* voting age population does raise some concerns as, for example, expatriates and other non-citizens of voting age might be included in the

population. However, because registering to vote is likely correlated with other determinants of turnout, using *turnout* raises additional concerns related to endogeneity. In addition, voter registration figures, according to IDEA, can be inaccurate or unavailable. These considerations lead me to choose VAP turnout as the key explanatory variable in the study. In practice, however, the two measures are very similar and the results of the study are robust to both.[5] The IDEA database also includes a number of other variables that I exploit as controls for the study in question. These include whether the election was presidential or parliamentary,[6] and Freedom House scores that measure political and civil rights. Lastly, the database maintains a dummy variable for whether a nation has compulsory voting laws, which I use as an instrument in the last part of my analysis.

Information regarding the top marginal tax rates comes from the OECD, but are compiled, for the years between 1974 to 2013, by the World Tax Database and The Tax Policy Centre. Tax rates for 2014 are readily available from the website of the OECD. I also obtain unemployment and population figures for each country in each year of the sample from the OECD.

I used the World Bank Open Data to obtain data on GDP per capita as well as annual growth in GDP. These are important control variables as they may have a direct bearing both on voter turnout as well as on government tax policy decisions. In addition, the World Bank data include figures for tertiary education enrolment rates of each country for each year of the study. Including this variable allows me to control for any education-related effects that may confound the analysis.

Table 1.2 provides descriptive statistics of the variables in the data set while Table A.2 in the Appendix describes the variables in more detail and provides their sources.

1.3.2 Trends in Voter Turnout and the Top Marginal Tax Rate

Figure 1.5 shows a steady downward trend in both voter turnout and the top marginal tax rates in 34 countries of the sample. Figure A.2 in the Appendix shows that similar trends exist within each country. To get an initial understanding of the strength of the relationship between the two variables, I plot, in Figure 1.6, the residuals from a

[5] Figure A.1 in the Appendix shows trends as well as differences between the two measures.

[6] Eleven countries in the sample have presidential and parliamentary elections in the same year. The analysis is insensitive to whether I use only the presidential election data or only their parliamentary election data. However, I choose, according to which election is more politically relevant, the most sensible election for each country. The results are insensitive to such choices and I report which elections from which country I use in Table A.4 in the Appendix.

Table 1.2: Summary Statistics: Entire Sample

	Mean	SD	Min	Max	Observations
Turnout (%)	77.0	12.4	42	96	356
VAP Turnout (%)	73.5	12.5	35	95	357
Top Tax Rate (%)	49.1	14.1	12	91	1,159
GDP Per Capita (USD 1,000s)	20.9	18.3	.59	117	1,331
Annual GDP Growth Rate (%)	2.8	3.1	-15	13	1,268
Unemployment Rate (%)	7.0	4.1	.0068	27	1,210
Population (millions)	32.2	50.4	.22	319	1,383
Tertiary Education Enrollment Rate (%)	42.0	23.3	1.1	110	1,219
Freedom House - Political Rights	1.2	0.6	1	5	349
Freedom House - Civil Liberties	1.4	0.8	1	5	349

regression of top marginal tax rates on year dummies and country fixed effects against the corresponding residuals of turnout. Plotting the residuals enables me to observe variation in the two variables when all country and time heterogeneity have been accounted for, thereby alleviating concerns of compositional effects that may arise in a straightforward scatter plot. As shown, the relationship between the two variables is positive.

1.3.3 OLS Estimates

To more formally assess the strength and direction of the relationship between voter turnout and the top rate of marginal tax, I estimate the parameters of the following specification:

$$TaxRate_{it} = \beta_0 + \beta_1 \cdot VAPTurnout_{it} + \mathbf{\Gamma} \cdot \mathbf{X_{it}} + u_{it} \tag{1.5}$$

Where $TaxRate_{it}$ is the top marginal tax rate in country i in year t and $\mathbf{X_{it}}$ is a vector of country-specific controls as discussed in subsection 1.3.1. The idiosyncratic disturbance term is expressed by u_{it} and is clustered at the country level. The variable of interest is β_1, which can be interpreted as the effect, in terms of percentage points, of a one percentage point increase in VAP turnout on top marginal tax rates across the OECD. The results are shown in Columns 1 and 2 of Table 1.3 and indicate that, accounting for a number of potentially confounding variables, β_1 is significant and positive, suggesting that a one percentage point increase in turnout is associated with an approximate 0.5 percentage point increase in the top rate of marginal tax.

To investigate the relationship further, I include year dummies and country

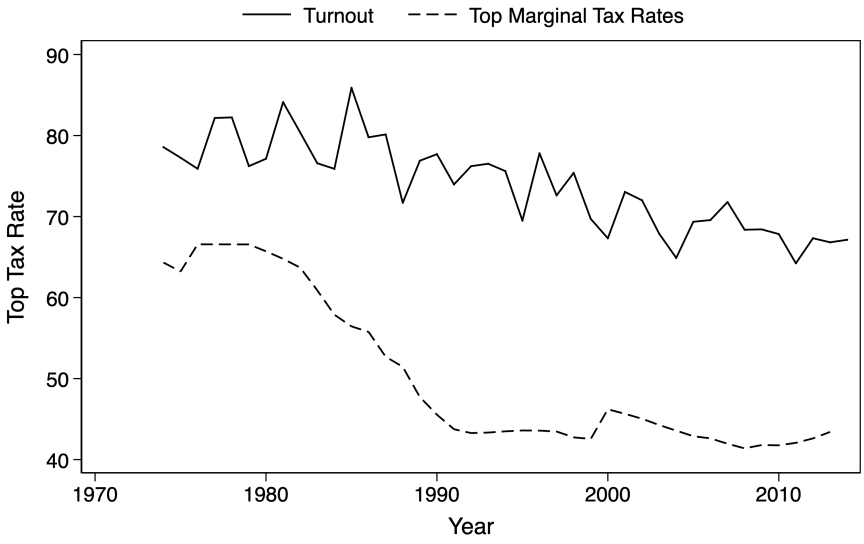

Figure 1.5: Top marginal tax rates and voter turnout in OECD Countries: 1974 to 2014

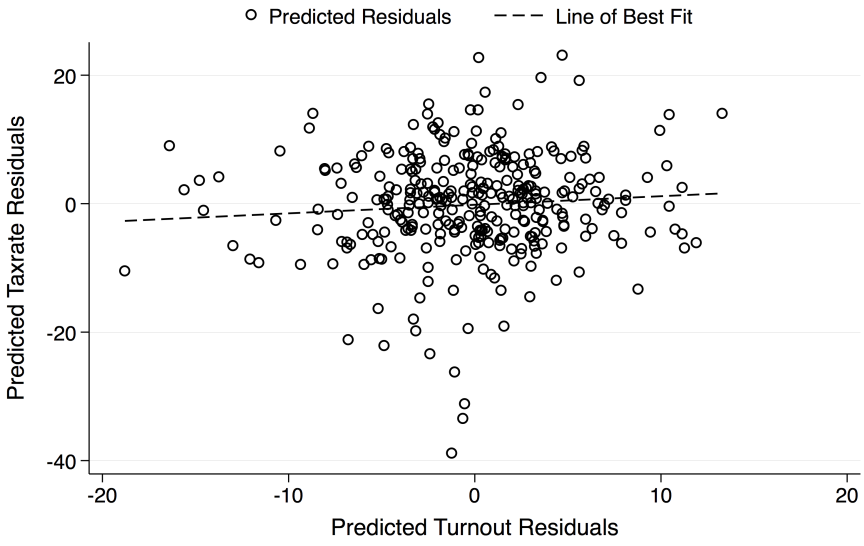

Figure 1.6: Scatter plot of tax rates and turnout in OECD countries

Note: This figure plots the residuals of a regression of top marginal tax rates on year and country fixed effects against the residuals from a regression of voter turnout on year and country fixed effects. The period of the sample is from 1974 to 2014.

Table 1.3: OLS and Fixed Effects Regressions

	Outcome: Top Marginal Tax Rates			
	(1)	(2)	(3)	(4)
VAP Turnout (%)	0.534***	0.590***	0.460***	0.258**
	(0.0852)	(0.105)	(0.104)	(0.127)
Full Set of Controls	No	Yes	Yes	Yes
Year Dummies	No	No	Yes	Yes
Country Fixed Effects	No	No	No	Yes
Observations	319	282	282	282
Number of Countries	34	34	34	34

Notes: The dependent variable is the top marginal tax rate in the 34 OECD countries. `VAP Turnout` is the percentage of the voting age population that voted in a given election in a given year. Control variables include population, an indicator for whether the election was presidential or parliamentary, GDP per capita, GDP growth rates, Freedom House scores for political and civil rights, tertiary education enrolment rates and the unemployment rate. Standard errors (in parentheses) are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

fixed effects to estimate the parameters of model 1.6. Year dummies, denoted by α_t, take into account any unobserved effects that are constant across countries but that may fluctuate over time, such as busts and booms in the global economy. Country fixed effects, indicated by δ_i, capture time-invariant, unobserved country heterogeneity and allow us to understand the within-country variation between the variables of interest.

$$TaxRate_{it} = \delta_i + \alpha_t + \beta_1 \cdot VAPTurnout_{it} + \mathbf{\Gamma} \cdot \mathbf{X_{it}} + u_{it} \tag{1.6}$$

Results are shown in Columns 3 and 4 of Table 1.3 and are similar to the baseline estimates. Column 3 shows the specification with year dummies whilst Column 4 shows the specification with both year dummies and country fixed effects. Column 4, the preferred specification, shows that, while the coefficient of interest does decrease from baseline OLS estimates, it remains positive, economically relevant and significantly different to zero. This suggests that voter turnout has a significant impact on top marginal tax rates even when unobserved time and country heterogeneity are accounted for.

These results indicate that voter turnout in year t has a contemporaneous effect on top marginal tax rates. Is this plausible? That is to say, if we believe the relationship is causal, do (or better yet, can) governments react that quickly to voter preferences?

Table 1.4: Fixed Effects Regressions with Time Leads

	Top Marginal Tax Rates in Year...			
	(1) *t+1*	(2) *t+2*	(3) *t+3*	(4) Election Cycle Avg.
VAP Turnout (%)	0.214*	0.138	0.191**	0.219**
	(0.114)	(0.0904)	(0.0871)	(0.106)
Full Set of Controls	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes
Observations	281	274	270	289
Number of Countries	34	34	34	34

Notes: The dependent variable is the top marginal tax rate in the 34 OECD countries with different time leads in Columns 1 to 3 and over the election cycle in Column 4. `VAP Turnout` is the percentage of the voting age population that voted in a given election in a given year. Control variables include population, an indicator for whether the election was presidential or parliamentary, GDP per capita, GDP growth rates, Freedom House scores for political and civil rights, tertiary education enrolment rates and the unemployment rate. Standard errors (in parentheses) are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To increase confidence in the reliability of the estimates thus far obtained, I regress top marginal tax rates in years $t+1$, $t+2$ and $t+3$ on turnout. I also regress the average value of the top marginal tax rate over the entire election cycle for each election in each country on turnout.[7] Results are shown in Table 1.4 which shows that the relationship remains significant at conventional levels and of similar magnitude to the baseline estimates.

To further increase confidence that the relationship observed is not spurious, I carry out a placebo test by regressing tax rates in years $t-1$, $t-2$ and $t-3$ on turnout in year t. The idea is that voter turnout provides a mandate for the tax policies of the *incoming* government; accordingly, we cannot expect elections in period t to have an effect on government policies that precede it, that is to say, in period $t-n$. Results are shown in Table 1.5 and show that, as expected, turnout has little to no explanatory power over tax rates in the years prior to an election. The coefficients are much smaller and, with the exception of Column 2, are not significantly different than zero.

[7] The total number of elections and the average length of the electoral cycle in each OECD country for the period of the sample are shown in Figures A.3 and A.4 in the Appendix.

Table 1.5: Fixed Effects Regressions with Time Lags

	Top Marginal Tax Rates in Year...		
	(1) *t–1*	(2) *t–2*	(3) *t–3*
VAP Turnout (%)	0.0323	0.158*	0.0895
	(0.115)	(0.0896)	(0.0944)
Full Set of Controls	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes
Observations	273	268	267
Number of Countries	34	34	34

Notes: The dependent variable is the top marginal tax rate in the 34 OECD countries with different time lags. `VAP Turnout` is the percentage of the voting age population that voted in a given election in a given year. Control variables include population, an indicator for whether the election was presidential or parliamentary, GDP per capita, GDP growth rates, Freedom House scores for political and civil rights, tertiary education enrolment rates and the unemployment rate. Standard errors (in parentheses) are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

1.3.4 Instrumental Variables

Thus far, the analysis has shown a statistically significant relationship between voter participation and the top marginal tax rate. Naturally, the correlation uncovered does not necessarily imply that the relationship is causal. Nonetheless, the correlation is robust to a range of controls related to the social, economic and political characteristics of the countries in the sample as well as to unobserved time and country heterogeneity. Results also hold when tax rates at various points in the future are regressed on turnout in time period t, suggesting that turnout is a factor in the redistributive policy of incoming governments. Still, one may be concerned about the existence of variables that are both correlated with turnout and determinants of tax rates, variables which would ultimately confound the analysis.

In this section, I undertake an instrumental variables strategy in an effort to further test the robustness and causal validity of the results. Specifically, I use an indicator variable for whether or not a country has compulsory voting laws as an instrument for voter turnout. In doing so, I follow Hoffman, Leon and Lombardi (2017) who use compulsory voting as an instrument to study the impact of turnout on social spending in Austria. The intuition is that compulsory voting laws have

no effect on tax rates except through voter turnout. The instrument is valid to the extent that it is (a) relevant to turnout and (b) orthogonal to the disturbance term of equation 1.5. Between 1974 and 2014, 10 of the OECD countries had, or continue to have, compulsory voting laws. Table A.3 in the Appendix illustrates the various experiences of the OECD countries with compulsory voting laws and makes clear that such laws do not change frequently.

The coefficient of the first stage (not reported) is positive and statistically significant at the 1 percent level. This result consistent with other studies that document the effect of compulsory voting on turnout (Hoffman, Leon and Lombardi, 2017; Quintelier, Hooghe and Marien, 2011). However, despite the precision of the first stage, the first-stage *F-statistic*, reported in the penultimate row of Table 1.6, is less than 10, indicating a weak instrument. The 2SLS results are also estimated imprecisely, as shown in Columns 1 and 2 of Table 1.6. Therefore, this evidence should be taken as simply as suggestive. Nonetheless, it is encouraging that the 2SLS estimates are comparable, terms of magnitude and sign, to those of earlier specifications. The reader will also notice that I use only the tax rates averaged over the electoral cycle as the dependent variable. Using other outcome variables (i.e. tax rates in years t or $t + n$) yields similar results to what is reported; I use the average outcome variable because it provides an indication of the effect of turnout over the entire electoral cycle.

A limitation of instrumental variable approaches is that the results rely, in part, on the exclusion restriction of the instrument which cannot be tested. In this particular context, the exclusion restriction implies that compulsory voting laws affect top marginal tax rates *only* through voter turnout. Although this claim cannot be empirically verified, I undertake a simple check to increase confidence that the exclusion restriction is satisfied. I run a reduced form regression on a sub-sample of the data, restricted to non-election years. The intuition is simple: if compulsory voting laws only exert influence on tax rates through voter turnout, then we expect to see no relationship between compulsory voting laws and tax rates in years for which there is no voting (i.e. non-election years). Column 3 of Table 1.6 shows the results and we see that, as expected, the coefficient on compulsory voting is not significantly different to zero.

A better understanding of the nature of compulsory voting laws can also provide evidence in support of the claim that the instrument satisfies the exclusion restriction. In this respect, two ideas stand out. First, although compulsory voting laws are enacted and repealed within some countries, such changes do not occur often. For instance, just four of the countries in the sample (Turkey, Italy, Chile and three states in Austria) changed their compulsory voting laws in the period between 1974 and 2014

Table 1.6: Instrumental Variables Regression

	Outcome: Top Marginal Tax Rates		
	(1) Top Tax Rate	(2) Election Cycle Average	(3) Top Tax Rate
VAP Turnout (%)	0.385	0.267	
	(0.647)	(0.510)	
Compulsory Voting			-1.393
			(3.313)
Control Variables	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	No
Observations	265	271	744
First Stage *F*–Statistic	7.22	7.28	—
Number of Countries	34	34	34

Notes: The dependent variable is the top marginal tax rate in the 34 OECD countries. `VAP Turnout` is the percentage of the voting age population that voted in a given election in a given year instrumented by an indicator for whether a given country has compulsory voting laws. Control variables include population, an indicator for whether the election was presidential or parliamentary, GDP per capita, GDP growth rates, Freedom House scores for political and civil rights, tertiary education enrolment rates and the unemployment rate. Standard errors (in parentheses) are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(Turkey enacted the law in 1983 while the other three repealed it at various times in the past 25 years).[8] Thus, the low rate at which such laws are altered within countries makes it difficult to understand how such laws could have a direct impact on tax rates which have changed so much over the 40 years of the sample.

A second point to bear in mind is *why* such laws were introduced in the first place. The precise reasons vary but in nearly all the cases of the OECD, the laws were introduced to accompany such changes to the political system as the expansion of suffrage or protection to civil rights. A research report prepared by the Electoral Commission of the United Kingdom (Younger, 2006), for example, makes clear that compulsory voting is introduced in various countries mainly to address low rates of turnout (Australia), to restore a sense of civic duty (Italy), or to accompany universal enfranchisement (Belgium). Compulsory voting laws, therefore, are not introduced—at least not explicitly—to serve economic purposes. Instead, they have been introduced to accompany other changes to the political system and as such, they seem to have little direct bearing on redistributive policy of the past or present. To the extent, therefore, that the exclusion restriction holds true, the overall sign and magnitude of the instrumental variables estimation confirm the analysis of this study that voter turnout does indeed have an impact on top marginal tax rates.

1.4. Discussion

Across a number of specifications, the results derived in this chapter suggest that a one percentage point increase in voter turnout leads to an increase in the top marginal tax rate in the order of 0.25 percentage points. In this section, I explore how large this effect is by carrying out some tax rate accounting. I also discuss the strengths and limitations of the paper and point out areas for future research.

If the correlations uncovered are indeed causal, then the 12 percentage point decrease in VAP turnout in the OECD countries between the late 1970s and early 2010s is associated with a 3 percentage point decrease in top marginal tax rates. Given that the average tax rate across the OECD has dropped by 23 percentage points, from 65 percent to 42 percent over the same time period, the fall in voter turnout explains some 13 percent of the fall in tax rates. Conversely, let us imagine a hypothetical situation in which voter turnout increased by 12 percentage points from the 1974–1975 average. This would imply participation of 90 percent of the total voting age population. A 12 percentage point increase in turnout would lead to a 3 percentage point increase in the top marginal tax rate to 69.6 percent. Interestingly, this number

[8] This presents certain challenges with regard to the precision of the 2SLS estimates and I will have more to say about this in the discussion.

resembles tax rate figures of several advanced industrial nations in the not-so-distant past. Germany, for example, experienced top marginal tax rates as high as 75 percent in the early 1950s and a 90 percent rate in the late 1940s (Alvaredo et al., 2013). The United Kingdom set top income tax rates as high as 98 percent in the 1950s, 1960s and 1970s while the United States levied a 91 percent tax on top incomes in the 1950s and 1960s and then relaxed the rate to 70 percent or more throughout the 1970s (Alvaredo et al., 2013; Piketty, 2015). A tax rate of some 70 percent on top incomes is also in line with estimates carried out by Piketty, Saez and Stantcheva (2014) who argue that if bargaining effects are moderately large, tax rates of 80 percent and higher may, in fact, be "consistent with a sensibly specified optimal tax model". Of course, the purpose of this section is not to argue a certain tax rate or even to suggest that the super-rich ought to pay more to the public purse. Instead, it is intended to illustrate the degree to which ordinary citizens could exert influence on public policy were they to be mobilised in greater numbers on election day.

Are the results derived in this chapter causal? It is difficult to say. On the one hand, a limitation of large, cross-sectional studies like this one is that sharp identification, from which causal inference can be made with greater confidence, is hard to come by. As mentioned in the subsection on instrumental variables, only four countries in my sample experience variation in the variable that serves as the instrument. This means that I rely on the variation of the instrument in four countries to predict the exogenous variation of turnout in a sample of 34 nations which may be one of the reasons why 2SLS results are estimated imprecisely. On the other hand, the results of the paper seem to suggest more than just a spurious correlation: estimates derived using OLS, panel methods and instrumental variables all point in the same direction: greater turnout has a positive effect on government redistributive policy. Given that this is, to my knowledge, the first study that investigates the relationship between turnout and tax rates, the results of this study, far from casting doubt on the nature of the relationship, call out for further research, perhaps using micro-level or sub-national data in order to benefit from sharper identification strategies.

1.5. Conclusion

This chapter adds to the literature that aims at better understanding the economic consequences of voter turnout. For the most part, this literature has focused on the relationship between voter turnout and government redistribution, as measured by how much governments allocate to spending on social ends such as health, education and pensions. These empirical studies confirm the results of the median voter theorem which asserts that, as voting approaches universal levels, governments tend more

towards redistributive policies.

In this chapter, I have built on these studies by exploring the relationship between voter turnout and a more direct measure of redistribution, namely, the top marginal tax rate. The theory motivating this study is similar to those of previous ones: if governments are intended to respond to the preferences of the median voter, a question arises as to what the economic consequences are when voter turnout declines and when that decline is marked along class lines. I have shown that increased participation in elections is associated with a positive and statistically significant impact on top marginal tax rates.

To assess the strength and direction of the relationship between the two, I undertook a number of empirical strategies. First, I included a range of social and economic characteristics that may confound the analysis. I then exploited the panel nature of my dataset to account for any unobserved country or time heterogeneity. As a final step, I employed an instrumental variables strategy in an effort to isolate only the exogenous variation voter turnout. I also take into account that governments may take time to respond to the mandate given to them by voters. As such, I modified my baseline specification in order to estimate the impact of turnout on tax rates in periods $t + n$ as well as over the entire electoral cycle. Across all specifications, results confirm my hypothesis that more turnout demands more redistribution.

Taken together, the results of this chapter underscore what may seem to be a self-evident truth: voting matters. Yet, in spite of the veracity of such a seemingly simple claim, fewer and fewer people are turning out to vote. This study shows that doing so is not without consequences.

Appendix A

A.1 Additional Figures

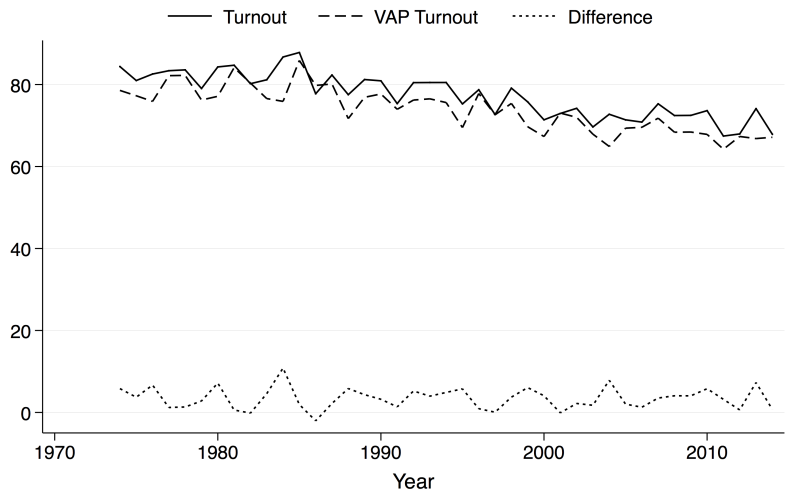

Figure A.1: Turnout and VAPTurnout in OECD countries: 1974 to 2014

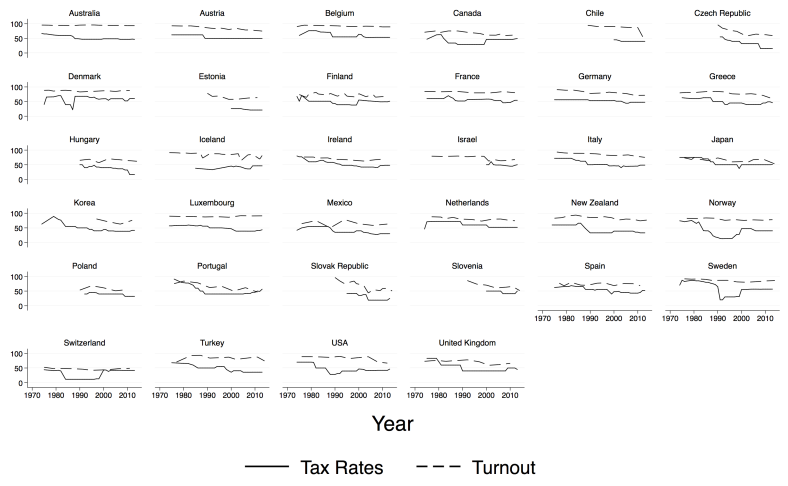

Figure A.2: Trends in Top Marginal Tax Rates and Voter Turnout in each OECD Country

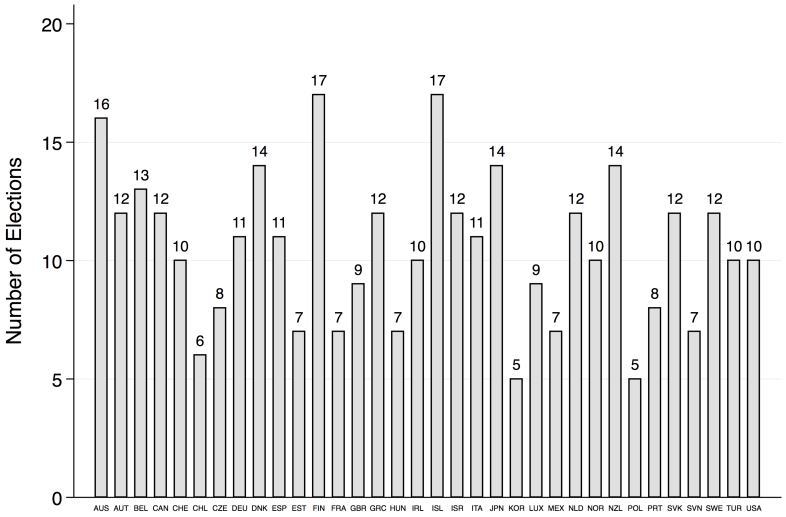

Figure A.3: Total number of elections over the period of the sample in each country

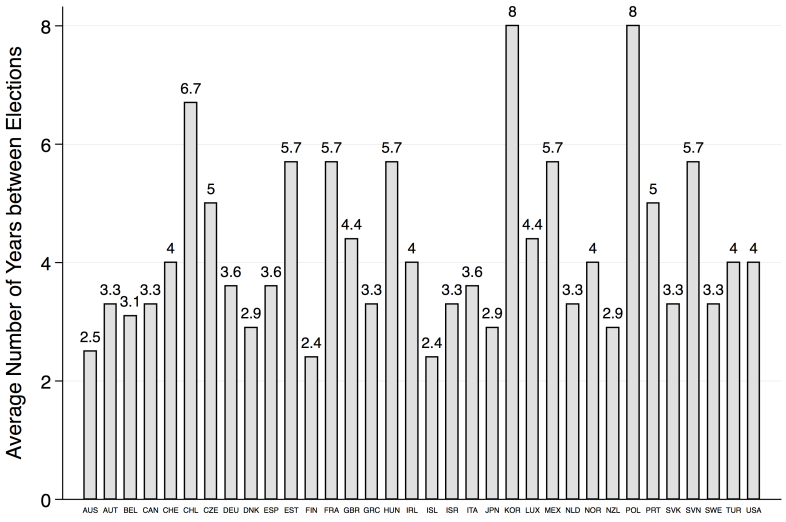

Figure A.4: Average number of years per election cycle in each OECD country

A.2 Additional Tables

Table A.1: Summary Statistics for the First and Last 5 Years of the Sample

	Mean	SD	Min	Max	Observations
Panel A: 1974 to 1979					
Turnout (%)	82.0	11.3	48	95	47
VAP Turnout (%)	78.5	11.7	41	95	47
Top Tax Rate (%)	65.8	11.0	40	91	136
GDP Per Capita (USD 1,000s)	6.1	3.5	.59	16	168
Annual GDP Growth Rate (%)	3.5	3.3	-11	13	158
Unemployment Rate (%)	4.3	2.7	.0068	9.9	140
Population (millions)	27.5	41.8	.22	225	204
Tertiary Education Enrollment Rate (%)	19.7	10.0	1.1	55	169
Freedom House - Political Rights	1.4	0.6	1	4	46
Freedom House - Civil Liberties	1.4	0.7	1	4	46
Panel B: 2010 to 2014					
Turnout (%)	70.1	13.0	42	93	49
VAP Turnout (%)	66.6	11.3	40	87	49
Top Tax Rate (%)	42.5	10.9	15	60	136
GDP Per Capita (USD 1,000s)	40.9	23.6	8.9	117	170
Annual GDP Growth Rate (%)	1.7	2.5	-9.1	9.2	170
Unemployment Rate (%)	8.6	4.7	3.1	27	166
Population (millions)	37.9	59.7	.32	319	159
Tertiary Education Enrollment Rate (%)	71.3	16.7	18	110	126
Freedom House - Political Rights	1.1	0.5	1	3	48
Freedom House - Civil Liberties	1.2	0.6	1	4	48

Table A.2: Description of Variables Used in the Dataset

Variable	Description	Source
Turnout	Calculated by dividing the number of people who voted by the total number of people who were registered for elections.	IDEA
VAP Turnout	Calculated by dividing the number of people who voted by the total voting age population.	IDEA
Tax Rate	Top marginal tax rate, expressed as a percentage.	OECD, World Tax Databse, Tax Policy Centre
GDP Growth	Annual percentage growth rate of GDP at market prices based on constant local currency. Aggregates are based on constant 2005 U.S. dollars.	World Bank OpenData
GDPPC	GDP per capita: gross domestic product divided by midyear population. Data in current USD.	World Bank OpenData
Population	Total population in a country.	OECD
Unemployment	Total number of unemployed people aged 15 and over	OECD
Tertiary Education Enrolment	Total enrolment in tertiary education, regardless of age, expressed as a percentage of the total population of the five-year age group following on from secondary school leaving.	World Bank OpenData
Freedom House - Political and Civil Rights	Freedom House index of political and civil rights: index ranges from 1 (free) to 7 (not free).	IDEA (taken from Freedom House)

Table A.3: OECD Countries and Compulsory Voting

OECD Countries with Compulsory Voting Laws		
Country	**Year Introduced**	**Enforced?**
Australia	1924	Yes. Non-voter faces a fine.
Austria	1929 to 1982 (all states)	Yes. Non-voters faced a fine.
Austria (Tyrol)	1929 to 2002	Yes. Non-voter faced a fine.
Austria (Vorarlberg)	1929 to 2004	Yes. Non-voter faced a fine.
Austria (Styria)	1929 to 1993	Yes
Austria (Carinthia)	1986 to 1993	Yes
Belgium	1893 (men) 1949 (women)	Yes. Possible imprisonment. Non-participation in 4 or more elections over 15 years leads to disenfranchisement.
Chile	1925 to 2012	Yes. Non-voter faced a fine and possible imprisonment.
France (Senate)	1950s or 60s	No
Greece	1926	No
Italy	1946 to 1993	No
Luxembourg	1919	Yes. Non-voter faces a fine but voting is compulsory only for those who are registered to vote and registering is not mandatory. Voting is voluntary for citizens over 70.
Mexico	Prior to 1946	No
The Netherlands	1917 to 1967	No
Spain	1907 to 1923	No
Switzerland (Schaffhausen)	1904	Yes. Non-voter faces a fine.
Turkey	1982	Yes. Non-voter faces a fine.
United States (Georgia)	1777	No
OECD Countries without Compulsory Voting Laws		
Canada, Czech Republic, Denmark, Estonia, Finland, Germany, Hungary, Iceland, Ireland, Israel, Italy, Japan, Republic of Korea, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Sweden, Switzerland, United Kingdom		

Source: International Institute for Democratic and Electoral Assistance (IDEA). Taken from its Compulsory Voting web page.

Table A.4: Elections chosen for countries with Presidential and Parliamentary Elections in the same year

Presidential Elections	**Parliamentary Elections**
United States	Republic of Ireland
France	Austria
Chile	Czech Republic
Mexico	Slovenia
South Korea	
Portugal	
Poland	

Chapter 2

The Political Economy of Immigrant Legalisation: Evidence from the 1986 IRCA*

2.1. Introduction

Undocumented migration has today become a hotly contested issue. This contest is, perhaps, fiercer nowhere than in the United States where, as of 2016, an estimated 11.3 million migrants reside in the country illegally, up from 5.8 million in 1996.[1] Given the polarised nature of the issue, we ask in this chapter what happens to the distribution of state and local finances when undocumented migrants are offered legal status through a nation-wide amnesty. We answer the question by focussing on the actions of incumbent state governors: how do they respond, in terms of public resource allocation, to the incentives created by documenting undocumented migrants? Do they allocate more public resources to the areas affected by amnesty and to what extent is such allocation intended to capture the votes of the newly documented migrants as opposed to other, perhaps competing, constituents?

The history of the United States offers a unique opportunity to study these issues. In 1986, the Reagan Administration passed into law the Immigration Reform and Control Act (IRCA) which legalised nearly 3 million undocumented migrants in the span of some three years and offered them a path to citizenship five years af-

*This chapter is joint work with Christoph Winter. We owe a debt of gratitude to Davide Cantoni for his support and advice throughout the project. In addition, we thank Nava Ashraf, Tim Besley, Marvin Deversi, Jeremiah Dittmar, Florian Englmaier, Francesco Fasani, Greg Fischer, Joshua Gottlieb, Yosh Halberstam, Benjamin Häusinger, Mitch Hoffman, Ethan Ilzetzki, Gerard Padró i Miquel, Andreas Peichl, Yona Rubinstein, Thomas Sampson, Andreas Steinmayr, Till Stowasser, Daniel Sturm, Munir Squires, Uwe Sunde, Ebonya Washington, Daniel Wissmann, Stephane Wolton, Noam Yuchtman as well as seminar participants at the University of Munich, SWEAT at the University of Toronto, the University of Guelph, the EBE Summer Meeting, CEMIR Junior Economist Workshop on Migration Research at the ifo Institute, the 2018 Congress of the Verein für Socialpolitik, the 20^{th} annual INFER conference, and the 2018 Econometric Society Winter Meeting for constructive comments and criticisms. All remaining errors are our own.

[1] Pew Research Centre, taken from http://pewrsr.ch/2oWlM93 Accessed 9 February 2018.

ter legalisation. In this study, we combine variation in legal status from the IRCA with administrative data from the Census of Governments in order to throw light on the ways in which documenting undocumented migrants affects the distribution of state and local finances. Our analysis is motivated by a model of distributive politics in which incumbent politicians react to changes in the electorate by adjusting their budget allocations to target the preferences of the newly enfranchised group so as to optimise (a) the welfare of the population and (b) their own re-election chances. Accordingly, we posit that governors, who play an important role in formulating the state budget, will allocate resources strategically to those counties most affected by the IRCA in an effort to win the political support of the newly documented migrants who were eligible to vote five years after legalisation.

Using a difference-in-differences regression framework, we compare the distribution of public finances—specifically, per capita inter-governmental transfers from state to local governments—in counties that experienced more per capita legalisations with those that received less both before and after 1986. Our baseline estimate suggests that counties with a greater share of IRCA-documented migrants received more per capita transfers from their state governments than those with a fewer share of such migrants. The result is not driven by differences in county social, economic or demographic characteristics and is robust to alternative specifications and samples.

To overcome potential geographical endogeneity associated with where undocumented migrants settle, we follow two approaches: first, we employ propensity score matching to identify a more comparable control group and second, we use the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. These tests confirm that the baseline result is not confounded by geographical factors.

Arguably, our result may be driven by mechanical or bureaucratic forces that oblige the state governor to better service the areas where the newly documented migrants reside rather than, as we posit, discretionary forces borne out of political calculation. To refute this competing explanation we analyse, in the second part, the sensitivity of inter-governmental transfers to political constraints. The rationale is straightforward. If the transfers of the state government are reflective of forces outside the control of the governor, then the result ought to be insensitive to political constraints faced by the governor. If, on the other hand, the transfers are politically motivated, the result ought to exhibit heterogeneity with respect to the various political constraints facing the incumbent. Consistent with this line of thinking, we find that counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition, enjoys line-item veto

power over the budget or is politically aligned with the state legislature. We also uncover heterogeneity along party lines: although both Republican and Democratic governors allocate significantly more resources to IRCA-affected counties, the effect increases by about half when the governor is a Democrat. It is, perhaps, unsurprising, therefore, that we also find that a governor's likelihood for re-election increases in the share of newly documented migrants in a state.

Finally, the IRCA provided a path to citizenship five years after legalisation. How plausible is it, then, that governors target their transfers to actually meet the needs, and win over the future political support, of the newly legalised as opposed to other, perhaps competing, constituents in a county? To address this issue, we first undertake a number of empirical checks to alleviate concerns that anti-migrant sentiment may drive our results. In this respect, we exploit data on a prominent anti-migrant ballot measure in California and find no relationship between state transfers and the interaction between a county's share of legalised migrants and its support for the ballot measure. More generally, we utilise survey data from the entire United States and find that, if anything, areas more affected by the IRCA actually experience improvements in attitudes towards both documented and undocumented migrants, further suggesting that anti-migrant sentiment is not confounding our results. Next, we turn our attention to local expenditure and find that expenditure in education increases in the share of newly documented migrants in a county. Hispanic individuals, as opposed to Caucasian ones, residing in these counties and who entered middle school after 1986 experience improvements in the likelihood of completing high school, suggesting that the additional resources a county receives on account of the IRCA were in fact targeted to the newly legalised as opposed to other voting blocs.

Together, these results point to a strong, nuanced political economy facet of immigrant legalisation. Although models of distributive politics generally predict that the expansion of franchise leads to greater resources being allocated to those who have a new-found political voice, it is more of an open question to what extent politicians allocate resources to capture this new "swing" vote as opposed to further solidify a core constituency. Our results indicate that, at least in the context of undocumented migration, politicians allocate resources primarily in an effort to capture the political support of the new swing vote rather than that of other groups.

In this chapter, we offer two contributions. First, we contribute to the literature that sheds light on the distributional effects of the expansion of voter franchise. Cascio and Washington (2014), for example, study enfranchisement of African Americans in the United States through the Voting Rights Act of 1965. They find that counties that removed literacy tests at voter registration in response to the law experienced

greater voter turnout among black voters which, in turn, increased the share of public spending directed towards them. In a similar vein, Miller (2008) shows that the enfranchisement of women in the United States was followed by immediate changes in legislative behaviour and substantial increases in public health spending at the local level. Examining the impact of electronic voting technology in Brasil, Fujiwara (2015) finds that the enfranchisement of lesser educated citizens affected government spending and increased health care spending, both of which are particularly beneficial for lower-income people. Naidu (2012), looking at a case of disenfranchisement, analyses the effects of the introduction of poll taxes and literacy tests in the 19^{th} century United States and finds that such measures lowered overall voter turnout with the subsequent effect of worse educational outcomes for black pupils and losses in annual income in the order of 15 percent for black labourers. We extend this literature to consider the case of undocumented Hispanic migrants in the United States, one of the largest disenfranchised groups in the country. Moreover, because of our rich data on state governors, we are able to examine some of the political mechanisms that lead governors to allocate resources in light of a sudden shock to the electorate.

Second, we contribute to the literature on the economics of legal status. In this respect, the IRCA has been used as a credible policy shock to identify the impact of legal status on various social and economic outcomes at the level of the individual migrant.[2] For example, Cortes (2013) shows that legal status helps migrants to obtain better educational outcomes whilst Kossoudji and Cobb-Clark (2002) and Pan (2012) use the IRCA to show that documenting undocumented migrants leads to an improvement in their wages, employment prospects and ability to speak English. We contribute to this literature by examining the influence of legal status directly on the distribution of public resources at the state and local level, bringing to light a dimension of undocumented migration that is highly debated yet relatively understudied.

The rest of this chapter proceeds as follows: Section 2.2 contextualises the study by discussing the historical background of the IRCA as well as the demographic characteristics of its applicants. In Section 2.3 we present a simple framework that guides our empirical analysis and the interpretation of our findings. Section 2.4 describes our data and explains institutional features associated with the budget-making process at the state level. Section 2.5 outlines our econometric methodology and, along with sections 2.6 and 2.7, presents our results. Section 2.8 concludes.

[2]Although some studies consider the impact of legal status on more aggregate outcomes. Baker (2015), for example, finds that counties with greater shares of IRCA applicants experienced a decline in crime rates.

2.2. Background

2.2.1 The Immigration Reform and Control Act

The Immigration Reform and Control Act (IRCA) of 1986 was, to date, the most extensive piece of legislation put forward by the United States government to address the question of undocumented immigration. The passage of the IRCA was by no means straightforward. It began in the 1970s when the legislative and executive branches of government considered various elements of comprehensive immigration reform. These efforts gained momentum when, in 1977, Congress appointed the Select Commission on Immigration and Refugee Policy which presented, in 1981, a proposal for immigration reform which was ultimately rejected. In the years that followed, several other proposals were put forward and variants of the IRCA were passed through either the Senate or the House but none was able to win complete approval until the 99^{th} Congress settled on and approved the IRCA on 17 October 1986 and which was signed into law on 6 November 1986.[3]

The purpose of the IRCA was to restrict the flow of undocumented migrants into the United States. It rested on three main pillars: an employer sanctions provision that made it illegal for employers to knowingly hire unauthorized workers; increased funding for border security to discourage new people from migrating illegally; and an amnesty programme intended to legalise various unauthorized workers (Chishti and Kamasaki, 2014).

While each of the components of the law was not without problems (in particular the employer sanction scheme which led to a great amount of fraud as well as workplace discrimination), the legalisation programme is generally regarded as the law's most successful provision. It provided two programmes for two distinct groups of unauthorized workers. First, the Legally Authorized Workers (LAWs, also knows as "pre-82s") under section 245A of the law enabled undocumented immigrants who resided in the country for an uninterrupted period from before 1 January 1982 to legalise (DHHS (December 1991), Cascio and Lewis (2018)). Second, the Special Agricultural Workers (SAW) under Section 210 of the law allowed applications from unauthorized migrants who could show that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (DHHS (December 1991); Cascio and Lewis (2018)). LAW applicants were eligible to apply

[3]The timing of the IRCA's passage in 1986 was indeed sudden and unexpected. Just days before its passage in Congress, "congressional leaders pronounced it dead, this time after more than fifteen months of hearings, legislative negotiations and debate" (Fuchs, 1990). Speaking to this idea, Representative Daniel E. Lungren (R-California) remarked on the day of the bill's passage that the IRCA was "a corpse going to the morgue, and on the way to the morgue a toe began to twitch and we started CPR again" (Fuchs, 1990). See Table B.1 for details on how Congress voted to pass the bill.

within a 12-month time frame extending from May 1987 to May 1988 whereas SAW applicants had an 18-month application period from 1 June 1987 to 30 November 1988 (DHHS, December 1991). On acceptance of their application, applicants were given temporary legal status under the title of *Temporary Resident Aliens* which could last for as long as 18 months. After this period, and upon successful completion of an English test and a civics test, applicants were given permanent resident status.

At the time of the Act, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Wasem, 2012). The law stipulated that both application periods (the 12 months for the LAW programme and 18 months for the SAW programme) were strictly enforced, which from an econometric point of view implies a relatively clean identification period. Indeed, by the end of the application period, roughly 3 million people applied for temporary resident status, of which 1.7 million comprised LAWs and 1.3 million comprised SAWs (DHHS, December 1991). By 1990, 94.6 percent and 58.7 percent, respectively, of LAW and SAW applications had received approval and for all intents and purposes, the legalisation programme of the IRCA ceased to generate newly legalised citizens after 1990 (DHHS, December 1991). Figure 2.1 shows the time trend of the stock of IRCA legalised migrants while Figure 2.2 shows the geographic distribution of IRCA applicants at the county level in 1992 for those counties for which data is available.[4] As shown, the majority of legalisations took place between 1986 and 1990 and in the states of California (970,895), Texas (351,646), Illinois (125,399), Arizona (70,488) and New Jersey (29,012). As shown in Figure 2.3, undocumented migrants applied for legal status in approximately 330 counties whereas the remaining counties received no such applications.

2.2.2 Demographic Characteristics of the Legalised

Figure 2.4 presents data from the December 1991 report to Congress from the Department of Health and Human Services which documented the characteristics of the legalised population. These data indicate that the newly legalised are predominantly of working age, healthy and with relatively few children. More than half and two-thirds, respectively, are single and male and the vast majority of applicants were engaged in full-time work. Fully 22 percent of all applicants reported a household income of over

[4]Of the 3,142 counties in the United States, our dataset includes IRCA information on 2,760 of them (and from all states except Alaska and Delaware). However, we do not observe every county in every year because some of the counties drop out in the later stages of the sample. As such, the actual number of treated counties varies slightly in the sample from 276 counties in 1999 to 332 in 1991/92. Restricting the analysis to only those counties that we observe throughout the entirety of the sample makes no difference to the results.

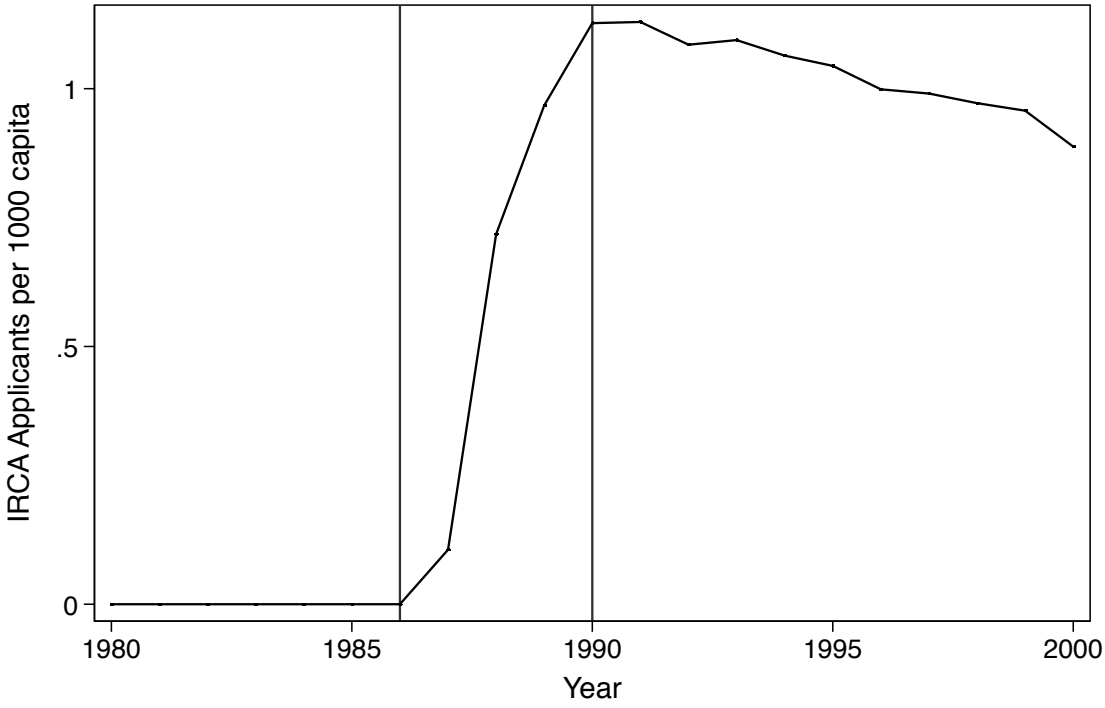

Figure 2.1: Stock of IRCA applicants at the county level per 1,000 capita

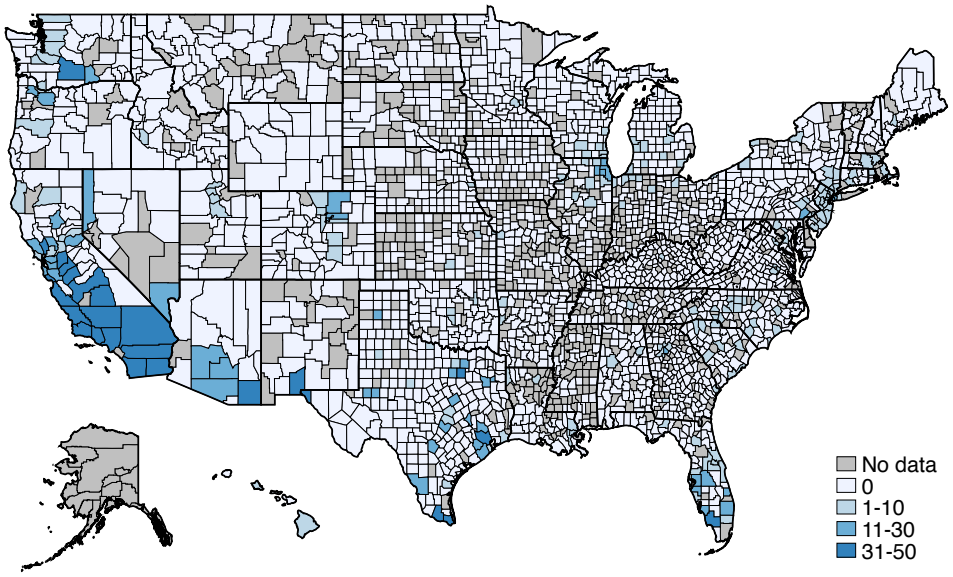

Figure 2.2: Number of legalized migrants per 1,000 county inhabitants in 1992

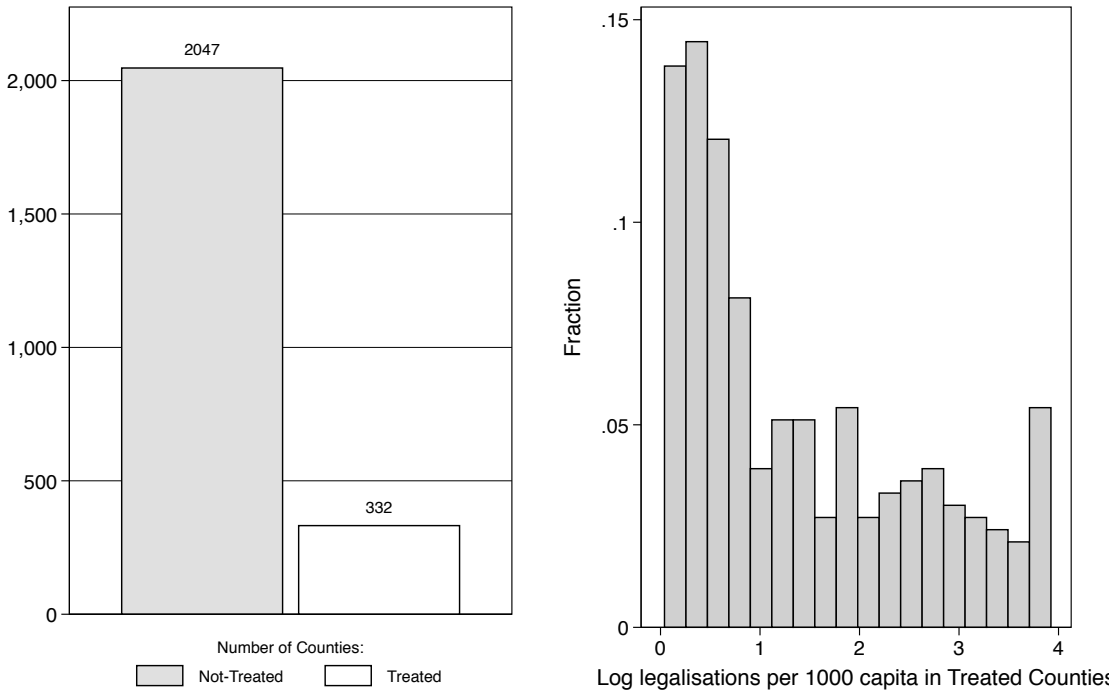

Figure 2.3: Distribution of legalised migrants

Note: This graph plots the distribution of the legalised migrants in 1992. The left panel indicates the number of treated (332) and control (2,047) counties whereas the panel on the right plots the distribution of treatment within treated counties. On a linear scale, the average treated county experienced 4.4 applications per 1,000 county inhabitants. The average population size of treated counties is just over 409,490, implying that on average 1,800 migrants received legal status in treated counties.

$600 per week; well over the poverty line, which, in 1989 stood at $6,311 for a single person ($121 per week) and $12,675 for a family of four ($244 per week) (Mosbacher and Bryant, 1991).[5] In fact, median take-home pay for IRCA applicants stood at $400 per week. Median household income in the population in 1989 stood at $23,745, or $456 per week. The report also makes clear that no more than 5 percent of the migrants reported not being able to work in the month prior to the survey. As such, IRCA applicants were, by and large, an economically active and self-reliant group earning somewhere between the poverty threshold and median income.

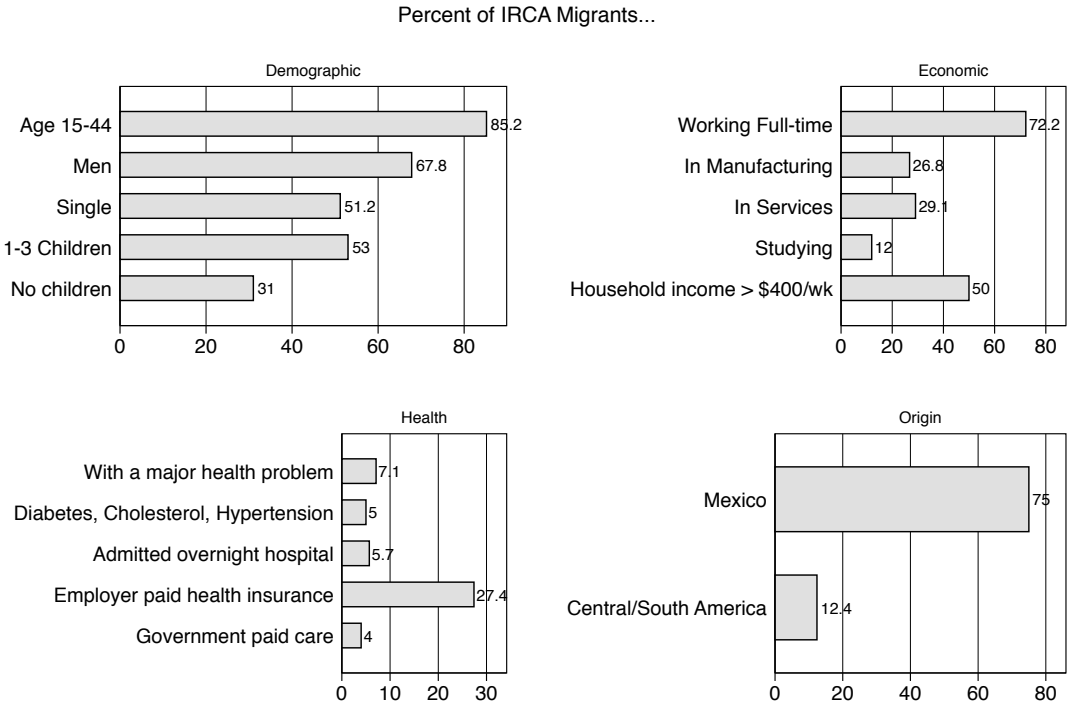

Figure 2.4: Socio-economic characteristics of the IRCA applicants

Notes: These are the characteristics of the IRCA migrants as reported by Congress in 1991.
Source: DHHS (December 1991)

2.3. Incumbent Politicians: A Framework

In this section, we present a simple framework to guide our empirical analysis. In this framework, an incumbent politician controls the distribution of transfers flowing

[5]The National Longitudinal Survey of the U.S. Bureau of Labour Statistics suggests the poverty thresholds in 1989 were even lower: $5,980 for a single person and $12,100 for a family of four. Taken from http://bit.ly/2tGnz8V, accessed in August 2017.

from the state budget to the various counties in the state. The politician is concerned both with the welfare of the population and her own re-election. A sudden change of legal status in a large and homogeneous group of residents in a county will thus change the politician's decision on how to distribute state resources so as to optimize her re-election chances and the welfare of the population. In our setting, therefore, we expect that the legalisation of a large group of mostly Hispanic people of lower socio-economic status will prompt politicians to offer policy over public resources so as to target the preferred outcome of the newly legalised group in the hope of winning their future political support.[6]

2.3.1 Politician Pay-off

We begin by modelling the objective function of an incumbent politician, P. In doing so, we adapt a model presented by Englmaier and Stowasser (2017) to consider a case of a state governor. For every county in the state, P transfers an amount of government assistance, $g > 0$, to the population at cost $C(g)$. As mentioned above, P is concerned with the utility of the residents, U, in every county as well as the utility derived from her expected vote share in elections, Ω.

Each county is composed of two types of inhabitants: already legal citizens, C, and newly legalised migrants, L. The population in each county is normalised to one such that the share of the population that is newly legalised is α and the share of already legal citizens is $1 - \alpha$. Only citizens can vote and voting decisions are based on local economic conditions, such as government expenditure on various public services. Voters base their voting decisions not just on local conditions immediately prior to the election but throughout P's term.

For every county in the state, P has a pay-off, Π, that is composed of three parts, shown in equation (2.1):

[6] In theory, there are two channels through which a policy shock such as the IRCA can affect the distribution of public resources (Persson and Tabellini, 2000). On the one hand, it might prompt a distribution of resources that is broad and general, for example, social benefits to all the members of some broadly defined group, such as the unemployed or the elderly. On the other, such programmes may be targeted and specific, aiming to benefit a more narrowly defined subgroup of the population. Broad redistributive programmes are those that appeal to the majority of the electorate and reflect their policy preferences. More targeted programmes, by contrast, impose little cost on the majority of the electorate but offer great incentives for its beneficiaries. Although state tax revenue does increase with the share of documented migrants in a state, the tax rate does not increase as a function of IRCA applicants and we show results later in the paper that suggest the transfers IRCA-affected counties receive from the state are, in fact, used for targeted local expenditures.

$$\Pi = \underbrace{(1-\alpha)U_C(g) + \alpha U_L(g)}_{1} + \underbrace{E \cdot \Omega[\phi\big((1-\alpha)U_C(g) + \alpha U_L(g)\big)]}_{2} - \underbrace{C(g)}_{3} \tag{2.1}$$

The politician is concerned with the well being of the population in each county. This is reflected in the first term of P's pay-off, $(1-\alpha)U_C(g) + \alpha U_L(g)$. We assume utility functions are concave such that $U_i'(g) > 0$ and $U_i''(g) < 0 \; \forall \; i \in \{C, L\}$. The only way in which P can improve the utility of the population is through her allocation of g. Because the characteristics of the documented migrants presented earlier, we take it as given that $U_L'(g) > U_C'(g) \;\; \forall \, g$.

We assume there are gains to staying in office. Accordingly, the second term, $E \cdot \Omega[\phi\big((1-\alpha)U_C(g) + \alpha U_L(g)\big)]$, captures the pay-off P obtains from re-election. E is a binary variable that is one when P is eligible for re-election and zero when P is a lame duck. In every election for which P is eligible to run, ϕ captures her expected vote share in that election which is a function of the well being of the population. We assume that ϕ is a linear function bounded between zero and one. The utility P derives from this expected vote share is captured by Ω. It is assumed that $\Omega(\phi)$, a strictly increasing, non-linear function with a negative third order derivative. Figure B.1 provides an illustration of what such a function might look like. As shown, the marginal utility derived from the expected vote share is the greatest at the inflection point of $\Omega(\phi = \phi_T)$ which represents the winning threshold.

Finally, the last term of equation 2.1 indicates the costs, $C(g)$, to the incumbent associated with allocating g to a given county. These capture the opportunity costs associated with distributing g among the different counties in a given state so as to remain within the budget constraint. Costs are sufficiently convex such that $\frac{\partial^2 \Pi_P}{\partial g^2} < 0$.

A rational incumbent politician thus maximises her expected pay-off over the allocation of state grants. That allocation is strongly affected by the share of newly legalised migrants, α, in a county. From this follows Predictions 1 to 3 which help guide our empirical analysis.

Prediction 1: The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Prediction 2: The optimal allocation of state aid is larger when P is eligible for re-election and less when (s)he is a lame duck.

Prediction 3: The optimal allocation of state aid becomes larger the closer the P's expected vote share is to the winning threshold.

Proofs can be found in the Appendix.

2.4. Data and Institutional Context

2.4.1 Data

The key explanatory variable in our study is a measure of the number of IRCA applicants per 1,000 county inhabitants in the United States for the period between 1980 and 2000. In the treated counties (i.e. those counties that received at least 1 application for legal status), this value ranged from as little as .04 to as many as 50 applications per 1,000 county inhabitants.[7] To carry out our analysis, we compiled a new dataset from a number of different administrative sources. Table 2.1 shows summary statistics of the main variables in our study according to whether they are in treated or non-treated counties.

[7] By 1992, treated counties received, on average, eight applications per 1,000 county inhabitants which translates into some 2,800 applications for legal status per treated county.

Table 2.1: Balance Table: Treated v. Untreated Counties in 1984

	Treated			Untreated			Difference	
	Mean	S.D	Counties	Mean	S.D	Counties	Mean	S.E
County Characteristics:								
Transfers (per capita USD1999)	144.0	[150.4]	307	147.4	[344.9]	1886	-3.37	(20.0)
Log of Transfers (per capita USD1999)	4.33	[1.31]	307	4.30	[1.31]	1886	0.027	(0.081)
Unemployment Rate	7.98	[1.84]	328	7.98	[2.27]	1892	-0.0017	(0.13)
Poverty Rate	11.9	[5.85]	328	16.8	[7.48]	1892	-4.94***	(0.43)
Population (1000)	377.3	[607.6]	307	31.7	[29.8]	1886	345.5***	(14.1)
Log of County Income	9.53	[0.19]	328	9.29	[0.18]	1892	0.24***	(0.011)
County Tax Revenue (Pc)	133.9	[116.1]	307	129.1	[130.3]	1838	4.81	(7.91)
Log of Total County Crimes (Pc)	-3.36	[0.64]	328	-3.67	[0.73]	1892	0.31***	(0.043)
1960 Population Foreign Born (%)	4.62	[3.79]	328	1.77	[2.21]	1892	2.85***	(0.15)
Governor Characteristics:								
Lame-Duck Governor	0.32	[0.47]	328	0.39	[0.49]	1892	-0.071**	(0.029)
State Has Term Limits	0.50	[0.50]	328	0.56	[0.50]	1892	-0.065**	(0.030)
Share Democratic Governor	0.66	[0.48]	328	0.78	[0.41]	1892	-0.13***	(0.025)
Governor Re-elected	0.43	[0.50]	30	0.65	[0.48]	209	-0.21**	(0.094)
Percent Votes Cast For D-President	38.2	[9.62]	328	36.7	[10.3]	1892	1.52**	(0.61)

Our measure of IRCA applications per county comes from Baker (2015) who, in turn, takes it from the Immigration and Naturalisation Service (INS). We also take from Baker (2015) measures of county poverty, population, unemployment and income, all of which are used as control variables in our analysis.

We aim at understanding the impact of the IRCA on the distribution of state and local finances and the sensitivity of this impact to political constraints. We thus add data on state and local finances taken from the US Census of Governments and use per capita inter-governmental revenues from state governments to local governments (counties, cities, municipalities) aggregated to the county as our dependent variable. We also utilise a host of governor related data including party affiliation of the governor, his or her name, an indicator for whether or not a governor is a lame duck and an indicator for whether (s)he enjoys line-item veto power in order to better understand the responsiveness of state politicians to the IRCA. These data are obtained from the Codebook for State Elections. We apply an instrumental variables strategy to confirm our OLS estimates, using the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. This variable is taken from the County and City Data Book prepared by the US Department of Commerce and the Census Bureau and made available by ICSPR under Study No. 7736.

2.4.2 Inter-governmental Revenue and The Budget-Making Process

Inter-governmental revenue (IGR)

The primary dependent variable is per capita inter-governmental revenue (IGR) received by local governments (counties, cities, municipalities, aggregated to the county) from state governments.[8] The Census Government Finance and Employment Classification Manual defines this variable as "[a]mounts received directly from the state government, including federal aid passed through the state government and state aid channelled through intermediate local government (e.g counties) which have no discretion as to its distribution. [It] includes state grants-in-aid, regardless of basis of distribution." Correspondence with staff at the Census Bureau confirms that "each state determines what specific funding sources (if any) are used for grants to local governments." and that "each state determines the nature, amount and distribution of state grants internally."[9]

Local governments have few major sources of local revenue, most notably prop-

[8] We use inter-governmental revenue, state aid and state transfers interchangeably.

[9] Personal correspondence with Michael Fredericks of the Local Government Finance Statistics Branch of the Census Bureau on 26 November 2018.

erty taxes and, to a much lesser extent, sales taxes. Inter-governmental revenue, therefore, is a type of budget support for local governments which comprises, on average, 30 percent of all local government revenue as shown in Figure B.2. Importantly, the local governments that receive this aid have "no discretion as to its distribution". State and federal governments decide to what areas these revenues are directed. For example, state aid in education is intended to "support of local schools" but excludes "State grants for libraries". The variable at our disposal is aggregate inter-governmental revenue at the county level and so it lumps together revenue intended for such areas as health, highways, education and public welfare.[10]

The budget-making process

Our main contention is that state governors use their budgetary powers to allocate more resources to newly documented migrants in the hope of winning their future vote. A crucial question is thus how much power governors actually exert over the budget-making process. We take up this question in this section and demonstrate that, in fact, governors have substantial influence in the formulation and implementation of the states fiscal priorities.[11]

For the vast majority of states, the budget-making process takes an entire year: it begins sometime in July or August and for all but four states, the fiscal year begins on 1 July.[12] The state budget office is responsible for the analysis and preparation of the budget on behalf of the governor.[13] The budget-making process begins when the state budget office requests proposals from, and provides guidance to, various state-level agencies. This guidance typically includes state spending targets, assumptions for inflation and priorities of the governor. In the fall, the various agencies submit their budget proposals to the governor who reviews them and provides additional direction. Once the governor's recommendations are incorporated, he or she presents the proposed budget to the state legislature in the winter season. After the legislature passes the budget, it requires the governors signature to become law.

[10] Although we only observe aggregate revenue, Table B.2 in the Appendix details what is and what is not included in the inter-governmental revenue received from the state and gives an indication as to what types of local activity these revenues support.

[11] The information in this section draws from the National Association of State Budget Officers report on the budget-making process NASBO (2015).

[12] In theory, 30 states operate an annual budget cycle and 20 operate a biennial budget cycle. In practice, however, most states employ a combination of both: in those states that operate a yearly budget, it is not uncommon for the governor to release spending recommendations for a two-year time horizon. States on a two-yearly budget cycle, by contrast, often prepare a supplemental budget which, in many cases, acts as a de facto yearly budget.

[13] The budget director is appointed directly by the governor in 34 states; in 13 states he or she is appointed by the department head and in one state the governor and department head share responsibility for this appointment.

Importantly, governors enjoy a number of powers over the budget-making process, including being able to spend unanticipated funds without legislative approval or to withhold appropriations from agencies within the executive, legislative or even judicial branches of government. Crucially, governors enjoy various forms of veto authority over the state budget. Depending on the state, governors have the authority to either veto the entire budget or specific line-items of it, a power which gives them great leverage over the prioritisation of the budget. Later in this chapter, we document heterogeneity in our results depending on the extent of veto power a governor enjoys.

2.5. Immigrant Legalisation and Inter-Governmental Revenue

2.5.1 The Evolution of IGR: Raw Data

Our aim is to understand the impact of documenting undocumented migrants on the distribution of inter-governmental transfers from state to local governments. The primary identifying assumption of our econometric model is that no other shocks occurred around the same time as the passage of the IRCA that correlate either with the number of legalised migrants in a given county or with the amount of inter-governmental revenue it received from the state. Prior to estimating the parameters of the model, therefore, it is informative to understand the evolution of IGR over time so as to lend credence to our identifying assumption. Figure 2.5 shows the trends in IGR as it appears in the raw data for the period between 1980 to 2000 in those counties that received applications for legal status with those that did not. As shown, the two county types developed along similar paths prior to the passage of the IRCA in 1986 and only after the passage of the law does one observe an appreciable difference between the two.[14]

As a more rigorous test for pre-treatment differences, we plot the coefficients of an event study as specified in equation 2.2:

$$y_{c,t} = \delta_c + \alpha_t + \sum_{j=1980}^{2000} \beta_j \left[T_c \times D_t^j \right] + \epsilon_{c,t} \tag{2.2}$$

Where $y_{c,t}$ is per capita inter-governmental revenue from state to local governments (in 1999 USD) in county c in year t; T_c is a binary variable set to one if a county

[14] In Figure B.3 we show the evolution of a number of other county covariates that make clear that the IRCA is not associated with corresponding shocks in such county characteristics as population, income, tax revenue or public school enrolment. This is because undocumented migrants were (and still are) included in population estimates, are eligible (especially their children) for basic public services such as health and education and, to some extent, pay tax as some undocumented migrants obtain illegal social security documents.

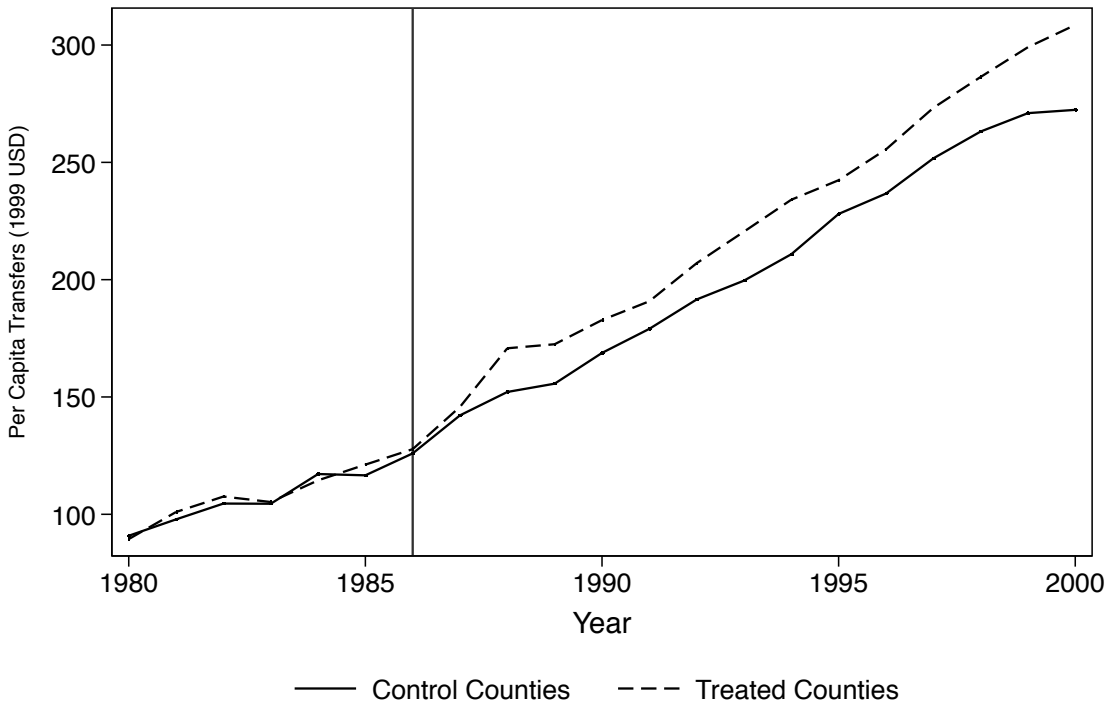

Figure 2.5: Evolution of IGR

Note: This graph compares per capita inter-governmental revenue received in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.

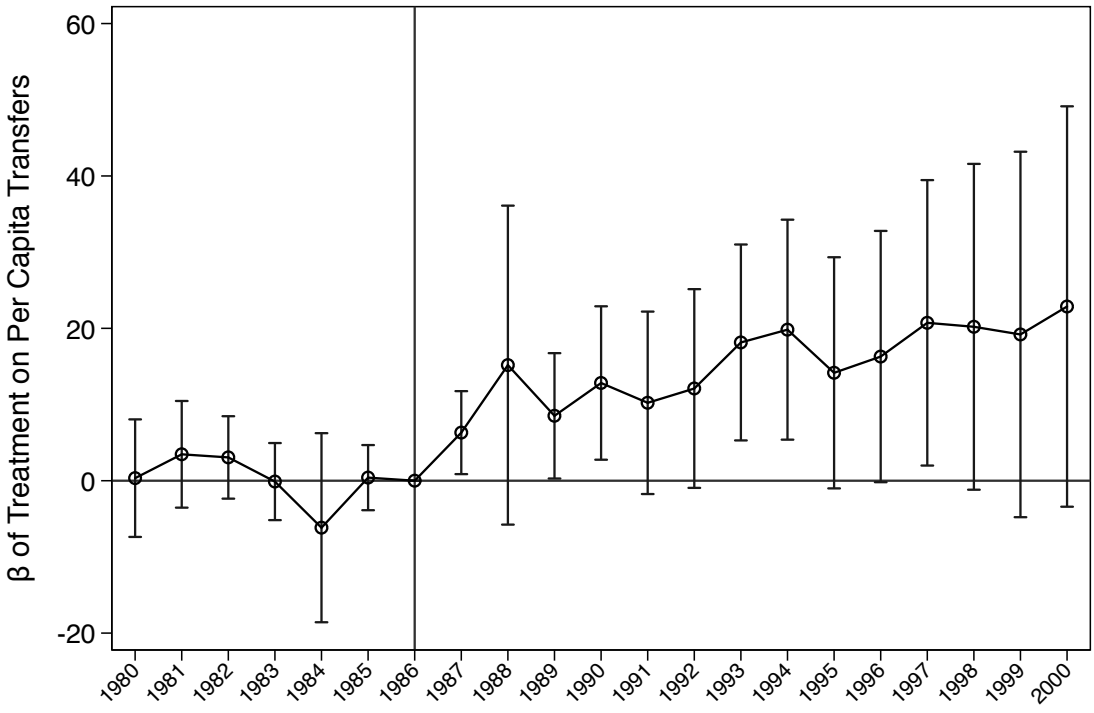

Figure 2.6: Treatment effect interacted with year dummies

Note: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 2.2. The regression only includes county fixed effects. The outcome, like Figure 2.5, is per capita inter-governmental revenue (in USD1999). Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 43,868$.

received one or more applications for legal status post-1986 and zero otherwise; and D_t^j is a dummy set to one when $t = j$ ($\forall\, j \neq 1986$). I capture county fixed effects by δ_c and time dummies by α_t while $\epsilon_{c,t}$ is an idiosyncratic disturbance term clustered at the county level. The results are shown in Figure 2.6, which indicate that the difference in transfers received between treated and non-treated counties shown in Figure 2.5 only becomes positive and significantly different to zero in the years after 1986, further increasing confidence in the reasonability of our identifying assumption.

2.5.2 Baseline Estimates

We impose more structure on model 2.2 in order to estimate the parameters of a difference-in-differences regression specification as detailed in equation 2.3.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \alpha_t + \zeta_{st} + \beta_1 \cdot (T_c \times P_t) + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{2.3}$$

Where $ln(y)_{c,t}$ is the natural log of per capita inter-governmental revenue from state to local governments (in 1999 USD) in county c in year t and δ_c and α_t are defined as before. The treatment indicator, T_c, is now interacted with a binary variable P_t, that is one if $t \geq 1986$ and zero otherwise. In addition, we include state-by-year fixed effects, ζ_{st}, to account for state-specific time-varying shocks that might affect legalisations and transfers, including governor specific characteristics or other state-year-level political or economic shocks. We include a vector of county-level covariates, $\mathbf{X_{c,t}}$, that includes poverty and unemployment rates, income and population. As before, $\epsilon_{c,t}$ is an idiosyncratic disturbance term clustered at the county level.[15]

The trends shown in the raw data are borne out in the regressions. Panel A of Table 2.2 shows our results across a number of variations of the model shown in 2.3 and we see precisely estimated coefficients of similar magnitude across a number of specifications. In Panel B, we estimate the same parameters but using a measure of treatment intensity as specified in equation 2.4. Here, $ln(IRCA+1)_{c,t}$ is the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) in county c in year t. The parameter of interest, β_1, can be interpreted as the elasticity of state transfers with respect to the cumulative number of per 1000 capita legalised applicants. All other parameters are defined as before.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \alpha_t + \zeta_{st} + \beta_1 \cdot ln(IRCA+1)_{c,t} + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{2.4}$$

[15]Because our unit of observation is the county and our treatment varies at this level, we cluster standard errors at the county. The results, however, are robust to clustering at higher levels, most notably the state. These results are captured in Table B.3 in the Appendix.

Table 2.2: Inter-Governmental Revenue on IRCA Legalisations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop $< 409,490$	(4) Matching	(5) Linear Trends
Panel A. Treatment Indicator					
Treatment $\times$ Post	0.0709***	0.0725***	0.0493**	0.138***	0.0556**
	(0.0183)	(0.0199)	(0.0193)	(0.0314)	(0.0241)
Panel B. Treatment Intensity					
Log legalisations	0.0610***	0.0929***	0.0448***	0.0688***	0.0462***
	(0.0143)	(0.0217)	(0.0158)	(0.0157)	(0.0173)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
County-Year Linear Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Panel A shows results when using a treatment indicator and Panel B shows results when using a measure of treatment intensity which is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Column 1 is our baseline estimate and suggests that counties affected by the IRCA received, on average, 7 percent more in per capita transfers than those that did not. Given that inter-governmental revenues make up, on average, 30 percent of local revenue, an increase in the order of 7 percent is significant. It corresponds to an increase of one and a half percentage points in the share of revenue received from inter-governmental sources. Using the measure of treatment intensity, the coefficient implies that a 1 percent increase in the number of per capita legalisations in a county is associated with an increase in per capita transfers of about .0610 percent. Within treated counties, moving from the 25^{th} to 50^{th} percentile of treatment intensity represents a 132 percent increase in per capita legalisations which translates to an increase of about 7 percent in per capita inter-governmental revenue.[16] Because treatment intensity is a much more precise measure of treatment, equation 2.4 is our preferred specification and henceforth we will use the cumulative number of per 1000 capita IRCA applicants (plus one) as our main explanatory variable.

To ensure that our results are not driven by confounding factors, we undertake a number of sensitivity checks. We begin by re-estimating the parameters of the model in a sample that omits the five most treated states which, in per capita terms, correspond to California, Arizona, Florida, New Jersey and Nevada.[17] As shown in Column 2, the results not only hold but become slightly larger, suggesting that legal status has a greater impact on the distribution of state finances in those states that do not absorb many newly legalised migrants. To alleviate concerns that the results are driven by very populous cities or counties—some of which may serve as so-called 'sanctuary cities'—we rerun the regression, in Column 3, on a sample restricted only to those counties with populations less than average county population size (i.e. 409,490) and obtain precisely estimated results, albeit of slightly smaller magnitude suggesting that the effect is strongest in more populated counties.

As shown in Figure 2.3, there are some 330 counties that received applications for legal status and just over 2,000 that did not. One might wonder, therefore, how comparable these two groups of counties are. To address these concerns, we use propensity score matching to generate a more comparable control group. That is, for every county in the sample, we generate, on the basis of its observable characteristics, a propensity score that indicates a given county's likelihood to be treated. Then, for every treated county, we match the nearest neighbour from the untreated counties to generate a more comparable control group. In Column 4, we rerun the model in this

[16] A very similar increase is associated with moving from the 50^{th} to 75^{th} percentile of treatment.

[17] Dropping the most treated states in terms of the absolute number of legalisations makes no difference to the results, nor does dropping the four states that border Mexico. These results are not reported.

matched sample and obtain results almost identical to those of the baseline.[18]

Finally, in Column 5, we rerun the baseline specification, adding to it county specific linear time trends. The idea here is to capture any differential trends with respect to the outcome variable that might arise over time for each county, trends which might render our identifying assumption implausible. This is the most demanding specification. That the result holds suggests that the relationship between immigrant legalisation and the distribution of state aid is a robust one.

2.5.3 Robustness Checks

In Table 2.3 we carry out a number of further empirical checks to test the strength of the relationship. In Column 1 we run a first-differences estimation using only two years in the sample: 1982 and 1992. The idea here is to skip intervening years to overcome issues with respect to timing of various sorts: different electoral cycles in different states, different budget response times and different IRCA application processing times. As shown, the legalisation variable maintains its predictive power over per capita inter-governmental revenue.[19] In Column 2, we use a county's 1980 population to carry out all per capita calculations as another way of ensuring population changes are not driving the results. To understand whether the relationship between immigrant legalisation and the distribution of state aid is linear or quadratic, we include a quadratic term of the key explanatory variable in Column 3. As shown, the linear variable retains its precision whereas the quadratic term enters imprecisely. In Column 4 we include quadratic year trends and in Column 5 we include additional county demographic controls, including the share of the population that is over 18 year of age, the share of the population that is Hispanic and the share of county households with children. Column 6 presents results from an instrumental variables estimation which will be explained in more detail in subsection 2.5.4.

In Table B.4, we replicate the baseline estimates using a $log - linear$ specification to demonstrate that the results are insensitive to the logarithmic transformation of the data. We choose a $log - log$ specification because (a) the legalisation variable is unevenly distributed and (b) an elasticity is easier for interpretation.

[18] Changing the number of neighbours up to 5 does not change the result. Figure B.4 in the Appendix shows the trends in inter-governmental revenue in treatment and control counties in the matched sample using nearest neighbour matching. The characteristics on which we generated the propensity score are county income, population, crime, tax revenue, poverty rate and unemployment in 1980. We drop state-year fixed effects to allow for the possibility that the best-matched control county for a given treated county may, in fact, lie in a different state. Matching within a state and leaving state-year fixed effects in the estimation does not change the results.

[19] Figure B.5 plots the coefficients from a number of such regressions, each using a different time period for the difference estimation.

Table 2.3: Robustness Checks

	Log of Inter-governmental Revenue (per capita)					
	(1) $\Delta y_{1992\text{-}1982}$	(2) 1980 PC	(3) IRCA^2	(4) Year^2	(5) Add. Controls	(6) IV
Log legalisations	0.0777***	0.0579***	0.102***	0.0463***	0.102***	0.199***
	(0.0201)	(0.0140)	(0.0363)	(0.0179)	(0.0177)	(0.0635)
Log legalisations^2			-0.0140			
			(0.0108)			
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	No	Yes	No
Linear Year Trends	No	No	No	Yes	No	No
Quadratic Year Trends	No	No	No	Yes	No	No
Observations	4,208	41,349	46,820	46,820	6,464	46,810
Number of Counties	2,104	2,211	2,686	2,686	2,407	2,685

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Column 2 carries out the analysis using per capita legalisation and per capita transfers calculated with 1980 county population in the denominator. Additional controls in Column 5 include the share of county population that is over 18, the share of county population that is Hispanic and the share of county households with children, which are only available for 1980, 1990 and 2000. Column 6 uses the share of foreign-born people in a county in 1960 interacted with year dummies as an instrument for log legalisations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2.5.4 Instrumental Variables

As a final empirical test to rule out endogeneity arising out of geographic factors associated with where the undocumented migrants settle, we use the share of a county's foreign-born population in 1960 as an instrument for the number of IRCA applicants post-1986. In doing so, we follow a number of other studies (Hildebrandt et al., 2005; Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010) that utilise historical rates of migration as an instrument for present levels.

Because the instrument in question is time-invariant we interact the 1960 foreign-born variable with year dummies in order to take advantage of the panel structure of our data. Doing so produces results that are positive and precisely estimated, though an order of magnitude larger than the baseline as shown in Column 6 of Table 2.3.[20]

2.5.5 Population Considerations

One may wonder whether our results are simply explained by a mechanical effect of having more people in the population eligible for social programmes. We rule out this possibility for three reasons.

First, while the IRCA legalised approximately 3 million people in some three years, it did not lead to a corresponding increase in the population. This is because estimates of the undocumented population are obtained from a residual of two other population measures: (1) the total foreign-born population (obtained through the Census) and (2) the legally resident population (known by the INS). The undocumented population estimate is the residual when (2) is subtracted from (1); hence population estimates undertaken by the Census Bureau are inclusive of undocumented migrants (Baker and Rytina, 2013). This fact is made evident in Figure B.3 which shows population growth in treated and untreated counties. As illustrated, neither type of county experienced appreciable growth in population in the years before or after the passage of the IRCA. Accordingly, even if funds were transferred by formula on the basis of a county's population, the fact that there is no population growth associated with the IRCA alleviates our concern that mechanical population forces drive our results.

Second, a feature of the IRCA was that it "barred" the newly legalised "from

[20] Another option is to run 13 cross-section regressions for each year between 1988 and 2000 where each variable is differenced from its 1982 value, as shown in specification 2.5, and the differenced variable is then instrumented in the cross-section. The regression coefficients are plotted in Figure B.6 and confirm, both in terms of precision and the timing of the effect, the baseline estimates shown in Figure B.5.

$$ln(y)_{c,t-1982} = \beta_0 + \beta_t \cdot ln(IRCA + 1)_{c,t-1982} + \Theta_t \cdot X_{c,t-1982} + \epsilon_{c,t} \tag{2.5}$$

participation in programmes of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status" (DHHS, December 1991). Moreover, given the demographic characteristics of the newly legalised discussed previously and that the children of undocumented migrants were already eligible for public services such as schooling pre-IRCA, we find it unlikely that our results are explained by mechanical increases as a result of social assistance eligibility criteria being satisfied at the state level.[21]

Third, the dependent variable used throughout our study is a measure of *per capita* transfers from state to local governments. If the policy was simply associated with a mechanical increase in transfers, we might expect the overall *level* of transfers to increase but there would be no reason, ex-ante, to expect any change in the amount of *per capita* transfers. That per capita transfers are a function of the number of legalisations in a county seems to suggest that the transfer activity we observe is more than a mechanical increase that might arise out of a transfer formula based on population considerations.

2.5.6 SUTVA

The stable unit treatment value assumption (SUTVA) holds that the potential outcome of a unit of observation is unaffected by the treatment status of other units. In this particular context, therefore, a question arises as to whether counties affected by the IRCA receive their transfers at the expense of those counties not affected by the law or whether these funds come from other sources. To better understand the nature of the treatment effect, and to understand whether SUTVA holds in this particular setting, we undertake two exercises.

First, there are four states in the sample that were unaffected by the IRCA. These are North and South Dakota, Vermont and Wyoming. As a first step, therefore, we run the baseline specification using the treated counties from treated states and only the control counties from these four control states. The idea here is that if the result is reflective of a distributive politics channel where the governor takes from control counties in order to give to treated counties, we should see no effect when we compare treatment and control counties from different states. To compare counties across state borders, we drop state-year fixed effects (and include year fixed effects instead) and generate the results presented in Table 2.4. They indicate that, by and large, the

[21] Later in the paper, we utilise Census of Government expenditure data to better understand the impact of legal status on various categories of local expenditure and find that the IRCA does not have a significant impact on local welfare expenditure (as shown in Figure 2.8).

Table 2.4: The IRCA and SUTVA

	Log of Inter-governmental Revenue (per capita)	
	(1) Full Sample	(2) Control States
Log legalisations	0.0988***	0.0782***
	(0.0143)	(0.0177)
Control Variables	Yes	Yes
Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes
Observations	46,826	10,771
Number of Counties	2,686	749

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). In Column 1 we exploit the full sample. In Column 2 we use only treated counties from treated states and the control counties from the four control states in the sample. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

treatment effect is not coming at the expense of control counties.

To probe this question further, we turn to state revenue data from the Census of Governments. Here we observe a state's revenues from various tax sources as well as from the federal government by way of inter-governmental revenue from the federal government to the state. The coefficient on per capita legalisations at the state level shown in Table 2.5 indicates that revenue from the state increases as a function of IRCA documented migrants in a state.[22]

2.6. Political Economy Mechanisms

The analysis in the preceding section demonstrated that the relationship between immigrant legalisation and the distribution of state finances is a robust one. In this section, we investigate to what extent the relationship is reflective of discretionary, political choices made by state governors and to what extent it is merely reflective

[22]To alleviate concerns that the result is driven by increased inter-governmental revenue from the federal government which simply passing through the state, the Census Bureau explains that "federal aid that is given to the state to then be distributed to local governments is normally considered state aid because states usually have discretion over the distribution."

Table 2.5: State Revenues and the IRCA

	Log of State Revenue From...		
	(1) Sales Tax	(2) Income Tax	(3) Federal Gov't
Log Legalisations, State	0.029**	0.022*	0.013*
	(0.013)	(0.011)	(0.007)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	853	853	916
Number of States	41	41	44

Notes: The dependent variable is the log of state revenue from various sources. `Log legalisations, State` is the log of the cumulative number of IRCA applications in a given state in a given year per 1000 state inhabitants (plus one). We control for state unemployment, population and income. Standard errors (shown in parentheses) are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

of mechanical, bureaucratic forces that oblige the governor to better service the areas where the documented migrants reside. To shed light on these issues, and on the mechanisms that drive the result, we turn our attention to the political constraints on, and the political context of, the state governor. The contention here is simple. If, on the one hand, the increases in per capita transfers associated with the IRCA are the result of mechanical forces, the results ought to be entirely insensitive to political context or constraints. If, on the other hand, the transfers are the result of discretionary choices made by state governors in an effort to bolster political support, then it is not unreasonable to expect state aid to display some sensitivity to political context.

2.6.1 Political Party Heterogeneity

We begin by investigating the sensitivity of our results to the party affiliation of the governor. Column 1 of Table 2.6 indicates that the per capita transfers a county receives in response to the IRCA policy are positive and significant and that this amount increases by about half when the governor is a Democrat as compared to when he or she is a Republican.[23] In Column 2 we test whether state governors give more to counties that are politically aligned with them, in the sense that a given county's political

[23]Figure B.9 and Table B.6 show, perhaps unsurprisingly, that the Democratic vote share in Presidential elections increases as the share of IRCA migrants in a county increases.

leaning (measured by its Presidential election results) align with those of the party of the governor.[24] As shown, state aid increases to a county affected by the IRCA regardless of whether the county's political leaning is aligned with that of the governor. Accordingly, these results confirm that the distributional impact of the IRCA is driven more by political factors at the state level.[25]

2.6.2 Term Limits and Election Cycles

Next, because our data includes the names of state governors, we are able to compare state-to-county transfers under a single governor over time as he or she faces different political constraints and election cycles. By way of example, we consider the transfers in just one state, Georgia, over the political career of one of its governors, Zell Miller (D), from 14 January 1991 to 11 January 1999. Governor Miller served two terms in office: from 1990 to 1994 and from 1994 to 1998. Georgia has a two-term limit constraint on the Governor.[26] Therefore Zell Miller was eligible for re-election in his first term but he was a lame duck in his second. Georgia comprises 159 counties of which we have data for 137. From among the counties for which we have data, eight received legalised migrants as a result of the IRCA and 129 did not. Figure 2.7 shows the trends in transfers during Zell Miller's tenure as Governor. As shown, the counties that received no legalisations experienced a steady decline in the amount of per capita transfers received. The eight counties that received legalisations, by contrast, exhibit a great deal of variation. In Governor Miller's first term, transfers to these counties increase only to drop off drastically in his second term when he is no longer eligible for re-election. The question that arises, therefore, is to what extent inter-governmental revenue differs as governors face term limits and how much of this difference is driven by the IRCA policy. Similarly, one wonders to what degree state aid fluctuates in the face of gubernatorial election cycles.

The regressions in Table 2.6 investigate these questions. In Column 3, we interact the legalisation variable with a binary variable that is one when the governor is eligible for re-election and zero when (s)he is a lame duck owing to a term limit

[24] We use Presidential election data as a proxy for Gubernatorial electoral returns because the Gubernatorial election data is available only as of 1990, after the variation in legalisations has ended. A county's Presidential election outcomes do follow its Gubernatorial outcomes quite well as shown in Figure B.8.

[25] In Table B.7 we explore whether the partisan composition of Congress has any explanatory power over the results. To this purpose, we interact the legalisation variable with indicators for whether a given state's Members of Congress or Senators were majority Democrats or not. As shown, the party affiliation of a states federal representatives has no explanatory power on the overall manner in which the state budget is distributed in response to IRCA.

[26] In the United States 26 states had term limits from 1980 to 1986, the majority of which were limited to 2 terms. Thereafter, the number of states with term limits increased to over 30, again the vast majority with a 2 term limit.

Table 2.6: Legalisation and Political Heterogeneity

	Log of Inter-governmental Revenue (per capita)			
	(1) Party	(2) Aligned	(3) Incentive	(4) Election Cycle
Log legalisations	0.0516***	0.0544***	0.0420***	0.0527***
	(0.0160)	(0.0143)	(0.0143)	(0.0157)
D-Governor × Log legalisations	0.0234*			
	(0.0121)			
Aligned		-0.000185		
		(0.00754)		
Aligned × Log legalisations		0.0139		
		(0.00936)		
Log legalisations × Incentive			0.0246**	
			(0.0107)	
Log legalisations × Election Year				0.0188***
				(0.00706)
County Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	46,393	46,820	41,807	30,694
Number of Counties	2,686	2,686	2,677	2,381

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). `D-Governor` is an indicator that is 1 if the party of the governor is Democratic and 0 if Republican. `Aligned` is an indicator that is 1 if the county's election results in the most recent Presidential election (Democrat or Republican) are aligned with the party of the Governor and 0 if not. `Incentive` is an indicator that is 1 if a governor is not a lame duck and 0 otherwise. `Election Year` is an indicator according to whether a governor is in an election year or not. The baseline effects of `D-Governor`, `Incentive` and `Election Year` are captured by state-year fixed effects and are thus unable to be estimated. The outcome variable in Column 4 is lagged by one year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

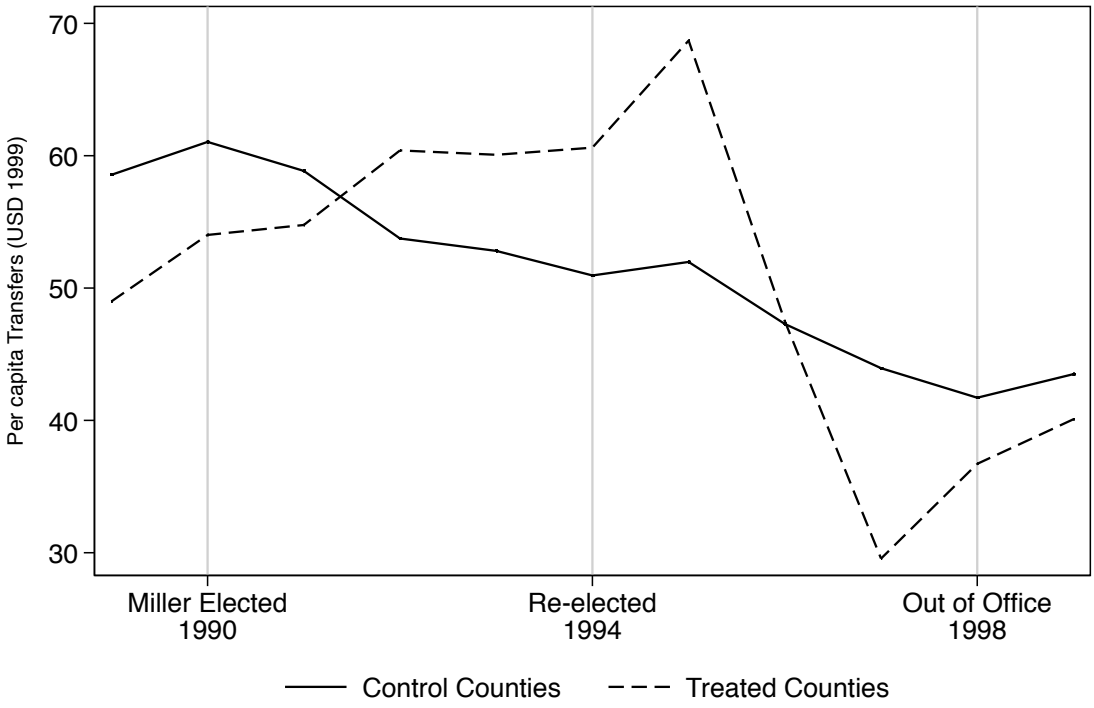

Figure 2.7: Transfers in Zell Miller's Georgia

Note: This graph plots inter-governmental transfers in 1999 USD in Georgia during Governor Zell Miller's time in office. Control counties are those that never received applications for legal status through the IRCA whereas treated counties did receive applications for legal status through the IRCA. Georgia comprises 157 counties of which we have data on 137: eight affected by the IRCA and 129 were not.

and the result indicates that the difference in transfers between lame duck and non-lame-duck governors is indeed positive and significant.[27] We carry out a similar analysis in Column 4, this time analysing sensitivity to the gubernatorial election cycle. Here, we lag the outcome variable by one year to better understand the dynamics of inter-governmental revenue in the year prior to an election. The result suggests that counties affected by the IRCA receive about 35 percent more in inter-governmental revenue in the year prior to a gubernatorial election.

2.6.3 Electoral Competition

To shed further light on mechanisms, we examine the sensitivity of transfers to electoral competition. The logic is similar to those of term limits. If the transfers we observe are discretionary, we would expect more resources to flow into those counties whose previous electoral races have been more competitive. To test this hypothesis, we generate the absolute value of the winning margin between Democrats and Republicans in the 1984 and 1988 Presidential election and identify those counties whose win margins are tighter than the tightest 25, 10 and 5 percent of the distribution in both elections. We then interact the legalisation variable with an indicator for whether a given county is competitive and compare this interaction across two time periods: 1984 to 1988 when the IRCA migrants were ineligible to vote and 1988 to 1992 when the IRCA migrants were eligible to vote. Results are shown in Table 2.7 and indicate that the impact of legalisation on state-to-county transfers is amplified in the post-1988 period when a given county is more politically contested.

2.6.4 Veto Power and State Legislatures

As mentioned earlier in the paper, governors exercise strong influence over the budget-making process in a given state. In this subsection, we focus on arguably the most influential of these powers: the line-item veto.[28] This accords with a range of theoretical and empirical literature that documents the growing importance of the state executive branch relative to the legislative branch in setting state priorities in general (Clych and Lauth, 1991) and in shaping the state budget in particular (Kousser and

[27] The advantage of this approach is that it allows us to trace the evolution of transfers over the course of a single governors term. However, one might be concerned that this specification does not allow us to estimate a governor's electoral incentive arising from the IRCA since the IRCA ceases to produce meaningful variation in the number of documented migrants after 1992. To address this, we re-estimate the parameter of interest, limiting the sample to the period only between 1989 and 1994 and compare governors who are lame ducks in this period with those who are not. Results are shown in Table B.8 and indicate that governors with an electoral incentive allocate significantly more resources than their lame duck counterparts as the share of documented migrants in a county increase.

[28] Figure B.10 illustrates how this power has grown stronger over time.

Table 2.7: Legalisation and Tightness-of-the-Race

	Log of Inter-governmental Revenue (per capita)		
	(1) Tightest 25%	(2) Tightest 10%	(3) Tightest 5%
Log legalisations	0.103***	0.0954***	0.0929***
	(0.0353)	(0.0324)	(0.0322)
Log legalisations × Tight25 × Post-1988	0.0698*		
	(0.0364)		
Log legalisations × Tight10 × Post-1988		0.0736*	
		(0.0377)	
Log legalisations × Tight5 × Post-1988			0.0688**
			(0.0350)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	18,585	18,585	18,585
Number of Counties	2,561	2,561	2,561

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). `Tight25`, `Tight10` and `Tight5` indicate, respectively, whether the outcome of the 1984 and 1988 Presidential election in a given county was more competitive (defined as the absolute difference between votes for the Republican and Democratic candidate) than those in the 25^{th}, 10^{th} and 5^{th} percentile of the competitiveness distribution. `Post-1988` is 1 for the period between 1988 and 1992 and 0 for the period from 1984 to 1988. The baseline effects of `Tight25`, `Tight10`, `Tight5` are captured by county fixed effects. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Phillips, 2012; Barrilleaux and Berkman, 2003).

In Columns 1 and 2 of Table 2.8, we split the sample according to whether or not the state governor enjoys line-item veto power and estimate the parameters of our baseline specification.[29] As shown, the legalisation variable has strong predictive power over inter-governmental revenue in those states where the governor has line item veto power.

In Column 3 of Table 2.8, we test the sensitivity of transfers to the relationship between the state governor and the state legislature. Although governors do enjoy increasing power over the state budget, legislatures still play a role. And if the result is discretionary, as opposed to mechanical, we might expect it to display important heterogeneities depending on the relationship between the executive and legislative branches of state government. Accordingly, we generate an indicator that is one when the party of the governor is aligned with the partisan majority of the state legislature and zero otherwise.[30] The result indicates that, although transfers increase as the share of IRCA applicants in a county increases when there is no political alignment between the governor and the legislature, the result increases by about 50 percent when there is political alignment, further underscoring the politically discretionary nature of these transfers.

2.6.5 Re-election Considerations

How might these political economy results impact a governors re-election chances? Our data includes an indicator for whether a particular governor was re-elected and we exploit this variable to understand whether the share of documented migrants in a state affects re-election chances in any way. Because this outcome varies at the state level over time, we can only include state and year fixed effects separately, denoted by γ_s and α_t respectively, but not jointly. Moreover, we allow for a five year lag on the key independent variable to take into account the time it takes for the documented migrants to earn the right to vote. Our specification is thus expressed in equation 2.6, where $\mathtt{R}_{s,t}$ is an indicator for whether the governor in state s has been re-elected in year t.

$$\mathtt{R}_{s,t} = \beta_0 + \gamma_s + \alpha_t + \beta_1 \cdot ln(IRCA + 1)_{s,t-5} + \Theta \cdot \mathbf{X_{s,t}} + \epsilon_{s,t} \tag{2.6}$$

The result is presented in Column 4 of Table 2.8. It suggests that as the share of documented migrants in a state increases, so too does the governors chances for

[29] Specifically, we compare states where the governor has line-item veto power to states where the governor has a simple veto, but not line-item veto.

[30] This includes when the state legislature is split or has a majority of the opposite party to the governor.

Table 2.8: Veto, State Legislatures and Re-election

	(1) Veto	(2) No Veto	(3) Alignment	(4) Re-elected
Log legalisations	0.0640***	0.00709	0.0535***	
	(0.0131)	(0.0301)	(0.0139)	
Log legalisations × Alignment			0.0245**	
			(0.0109)	
Log legalised$_{t-5}$, State				0.247***
				(0.0545)
County controls	Yes	Yes	Yes	No
State controls	No	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	No
State-Year Fixed Effects	Yes	Yes	Yes	No
State Fixed Effects	No	No	No	Yes
Observations	41,449	5,356	46,820	6,091
Number of Counties	2,555	670	2,686	2,239

Notes: In Columns 1 to 3, the dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. In Column 4, the outcome is a binary variable that is 1 if the governor is re-elected and 0 otherwise. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one) and `Log legalised`$_{t-5}$`, State` is similarly defined, but aggregated at the state level and with a five year lag. Column 1 restricts the sample to those Governors who enjoy line-item veto power and Column 2 restricts the sample to those Governors who enjoy no such power. `Alignment` is an indicator that is one when the party of the Governor is aligned with the partisan majority of the state legislature and 0 when it is not. The baseline effects of `Aligned` is captured by state-year fixed effects and thus cannot be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. State controls are the same, but aggregated to the state level for the specification in Column 4 and also include the party of the governor. Standard errors (shown in parentheses) are clustered at the county level in Columns 1 to 3 and at the state level in Column 4. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

re-election, lending further credence to the political economy nature of our baseline results.[31]

2.7. Capturing the Vote of the Newly Legalised

Thus far, we have demonstrated a robust relationship between immigrant legalisation and the distribution of public resources. Governors allocate more resources to counties as the share of newly documented migrants in those counties increase, an allocation which is particularly sensitive to the political context of the incumbent. Until now, however, we have implicitly assumed that the inter-governmental revenue is intended to capture the vote of the migrants who obtained legal status through the IRCA. Of course, because the inter-governmental revenue we observe is aggregated at the county level, we are unable to verify with certainty which constituents these monies are actually intended to reach.[32] In this section, therefore, we present a number of pieces of evidence that demonstrate that it is indeed the IRCA migrants who motivate the state governor.

2.7.1 Attitudes Towards Migrants

Like today, undocumented migration was a politically charged issue at the time of the IRCA. A notable opponent of the IRCA, and of undocumented migration more generally, was Governor Pete Wilson, Republican governor of California, who ran a campaign of fear and anti-migrant propaganda.[33] In his 1994 re-election campaign, Governor Wilson pinned his hopes onto Proposition 187, the "Save Our State" bal-

[31] One question that might arise is how visible these transfers are in the sense of their ability to influence public opinion about the incumbent. In this respect, an assumption in our conceptual framework is that voting decisions in a county are based on local economic conditions and that voters base their decision not just on local conditions immediately prior to the election but rather throughout the term of the governor. In the terminology of Elinder, Jordahl and Poutvaara (2015), voters in our framework base their decisions *retrospectively* (i.e. based on the implemented policies of the incumbent) rather than *prospectively* (i.e. based on the promises candidates make). In Table B.9 we investigate how the allocation on state aid affects local spending and the coefficient suggests that across all categories of local spending, the elasticity of such spending with respect to state aid is positive and precisely estimated, indicating that these transfers are visibly felt at the local level.

[32] Moreover, because this revenue is dedicated to such purposes as health and road improvements, the governor can use it to win over several constituents and not just a single one. Indeed, a key difference between the governor and his or her legislative counterparts is that the governor can shape a states fiscal priorities so as to build winning coalitions from among otherwise competing constituents; legislators on the other hand often vote over single issues, increasing the likelihood of generating 'winners' and 'losers' from any given vote (Cascio and Washington, 2014).

[33] In a dramatic re-election advertisement, Governor Wilson states "I'm suing to force the Federal Government to control the border and I'm working to deny state services to illegal immigrants. Enough is enough." (Transcribed from the Television Ad which can be found at: `https://www.youtube.com/watch?v=lLIzzs2HHgY`. Accessed 8 March 2018.

lot initiative, and the Republican Party offered ideological and financial backing to see the proposition go through. Proposition 187 prohibited undocumented migrants from using non-emergency public services and required the providers of such services to immediately report undocumented migrants for deportation. It was passed by California's voters only to be struck down by a federal court. The proposition, and Wilson's campaign to support it, was highly controversial and left somewhat of an enduring legacy. Bowler, Nicholson and Segura (2006), for example, find that racially charged ballot initiatives in California—and specifically Proposition 187—are significantly associated with a shift in political support away from the Republican party and towards the Democratic party on behalf of non-Hispanic white voters as well as Latino voters.

In light of this political context, it seems reasonable to ask to what degree our results are actually driven by governors catering to anti-migrant sentiment arising out of the IRCA rather than to the needs of the documented migrants themselves. We examine this question first by quantifying the impact of Governor Wilson's term in office and of Proposition 187 on state aid. In Column 1, therefore, we restrict the sample to consider only California during the eight years for which Governor Wilson was in power (1991 to 1998) and exploit variation in county-level voting outcomes for Proposition 187. Forty-seven of California's 55 counties voted for the Proposition and eight rejected it and the results varied from as little as 29 percent in favour to as much as 77 percent. Perhaps unsurprisingly, counties more affected by the IRCA received less inter-governmental revenue during Governor Wilson's tenure. However, this result wiped away and made positive for counties with a vote share of 49.5 percent or higher. However, neither of the coefficients are precisely estimated, which suggests that the impact of immigrant legalisation on state aid is not, in California at least, confounded by anti-migrant sentiment. In Column 2 we estimate the parameters of the baseline specification excluding California, the state with the strongest expression of anti-migrant sentiment at the time and the results hold. In the years following proposition 187, ten other states passed ballot initiatives or laws similar to that of Proposition 187.[34] Dropping these states from the analysis—presumably the states where governors had the strongest incentives to cater to anti-migrant sentiment—does not alter the results in any meaningful way.

[34]These are Arizona, Colorado, Florida, Georgia, Illinois, Nevada, New Mexico, New York, Oklahoma and Texas as reported by Richard Lacayo (December 19, 2004) in the following report: `https://ti.me/2PbD7YE`.

Table 2.9: IRCA and Anti-Migrant Sentiment

	Log of Inter-governmental Revenue (per capita)		
	(1) Wilson	(2) No Cali	(3) No Anti-Migrant States
Log legalisations	-1.844	0.0834***	0.0579**
	(1.601)	(0.0182)	(0.0238)
Log legalisations × Prop 187 VS	0.0372		
	(0.0241)		
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	428	45,690	32,232
Number of Counties	54	2,632	1,864

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). `Prop187 VS` is the county vote share for Proposition 187. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column 3 excludes the 10 states (plus California) that passed ballot initiatives or laws similar in spirit to those of Proposition 187. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: The IRCA and Attitudes towards Migration (GSS Survey)

	Attitudes on Undocumented Migrants			Attitudes on Documented Migrants	
	(1) Given Work Permits	(2) Work Hard	(3) Deported	(4) Increase Crime	(5) Take Jobs Away
Log legalisations	1.669**	0.540***	-1.818*	-3.673*	-3.760*
	(0.718)	(0.130)	(0.815)	(1.698)	(1.858)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	No	No
Region Fixed Effects	No	Yes	No	No	No
Observations	873	16,179	705	581	639
Number of Regions	9	9	9	9	9
Years in Sample	1994	1980 — 1998	1996	1996	1996

Notes: This table uses General Social Survey (GSS) data merged with the legalisation data. `Log legalisations` is the log of the cumulative number of IRCA applications in a given region in a given year per 1000 region inhabitants (plus one). The outcome variables are all binary indicators on various attitudes towards documented and undocumented migrants. Control variables include individual income, employment status, marital status, age, educational attainment and race. Standard errors (shown in parentheses) are clustered at the region level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As a more general check, we merge the legalisation variable with data from the General Social Survey (GSS), which includes a range of questions on attitudes towards migration. As indicated in the various columns of Table 2.10, a higher share of legalised migrants in a region tends to improve, not worsen, attitudes towards documented and undocumented migrants. Individuals residing in such regions tend to think undocumented migrants work hard, deserve work permits and ought to be protected against deportation. Such individuals are also of the opinion that documented migrants neither increase crime nor take jobs away from native citizens. Given these attitudes, and given the fact that the IRCA was not associated with an influx of migration but rather a change in the legal status of already resident migrants, we find it unlikely that state aid in IRCA-affected counties is intended to satisfy nativist sentiment or general opposition to the amnesty.

2.7.2 The IRCA, Local Expenditure and Hispanic Outcomes

Finally, we turn our attention from county revenue to county expenditure in an effort to better understand in which areas and, potentially, on which constituents county revenue is spent. Figure 2.8 plots event study estimates when the legalisation intensity in 1992 is interacted with year dummies in regressions with various categories of local expenditure as the outcome. These figures suggest that the IRCA led to increases in local expenditure in the areas of health, education and welfare but that these increases are estimated with precision for education expenditure beginning in 1991.

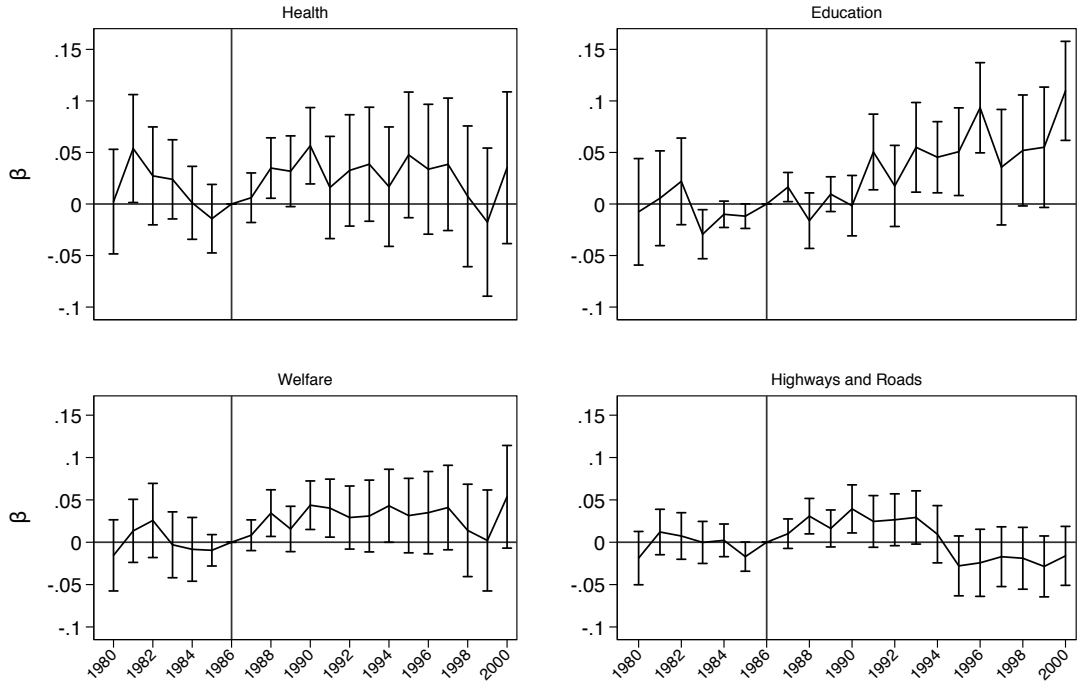

Figure 2.8: Event study estimates of local expenditure on legalisation

Note: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications in a given county per 1000 county inhabitants (plus one) in 1992 when it is interacted with year dummies. The outcome variables are the log of per capita county expenditure in health, education, welfare and highways and roads. The regressions control for poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 34,840$ for all regressions.

To understand whether these educational expenditures were intended to benefit the newly documented migrants and/or their families, we calculate race-specific high school completion rates to test whether the counties that were affected by the IRCA also experienced improvements in Hispanic high school completion. To carry out this exercise, we obtain data from the 2010 decennial census in order to estimate the impact of the IRCA on an individual's educational outcomes. Rather than compare individuals in treated and non-treated counties before and after the passage of the IRCA, we now compare individuals in treated and non-treated counties in cohorts that entered middle school before the passage of the IRCA (and hence were less likely to benefit from additional educational expenditure) with those cohorts that entered middle school after the IRCA passed (and hence were more likely to benefit from additional funds). Accordingly, we construct 13 middle school entry cohorts from

1980 to 1992. An individual in the 2010 census is placed in a middle school entry cohort depending on which year he or she was 12 years of age. The specification is detailed in equation 2.7, where $\mathtt{H}_{i,c,mse}$ is an indicator if individual i in county c and in middle school entry cohort mse has 12 years of education or more. County and middle school entry cohort fixed effects are captured by δ_c and ψ_{mse}, respectively and D^j_{mse} is an indicator that is one when $j = mse$ and zero otherwise $\forall\ j \neq 7$. All other terms are defined as before.

$$\mathtt{H}_{i,c,mse} = \beta_0 + \delta_c + \psi_{mse} + \sum_{j=1}^{13} \beta_j \left[ln(IRCA + 1)_{c,1992} \times D^j_{mse} \right] + \epsilon_{c,mse} \tag{2.7}$$

We first run the specification on a sample of only Hispanic individuals and then on a sample of only Caucasian persons and plot the corresponding coefficient, β_j, as shown in Figure 2.9. This coefficient estimates the change in the slope of high school completion between individuals in high and low treatment intensity counties across various middle school entry cohorts. The event study estimates indicate that for Hispanic persons, residing in a county affected by the IRCA led to a positive and significant impact on that person's likelihood of completing high school, provided they entered middle school in 1991 or 1992. Indeed, there is no distinguishable difference in the likelihood of completing high school between individuals in high-treated and low-treated counties if they began middle school prior to this time. The timing of this effect suggests that the increased high school completion rates arise not just from legal status but from additional resources that these counties receive for education. For Caucasians individuals, by contrast, residing in an IRCA-affected county has no distinguishable impact on high school completion probability, regardless of when they began middle school. These results lend further credence to our hypothesis that state politicians targeted newly documented migrants.[35][36]

[35] The results are even stronger when comparing individuals across different elementary school entry cohorts (i.e. the year in which an individual was six years of age) and are shown in Figure B.13.

[36] In the Appendix, we also try to understand the electoral relevance of the IRCA. To address this, we obtain individual-level data from the November voter supplement of the CPS which includes indicators for whether a person voted in the November election. Our aim is to understand how individual voting is affected by both the IRCA and inter-governmental revenue. We thus plot the marginal effect of the IRCA on the propensity of an individual to vote along the distribution of inter-governmental revenue as shown in Figure B.12. Importantly, though, we have data for only three periods: 1996, 1998 and 2000 because the CPS does not include county identifiers for earlier time periods. As such, these results include no pre-treatment observations and ought to be taken as suggestive. Table B.10 presents the results of the margins plot in table form along with other results from the CPS.

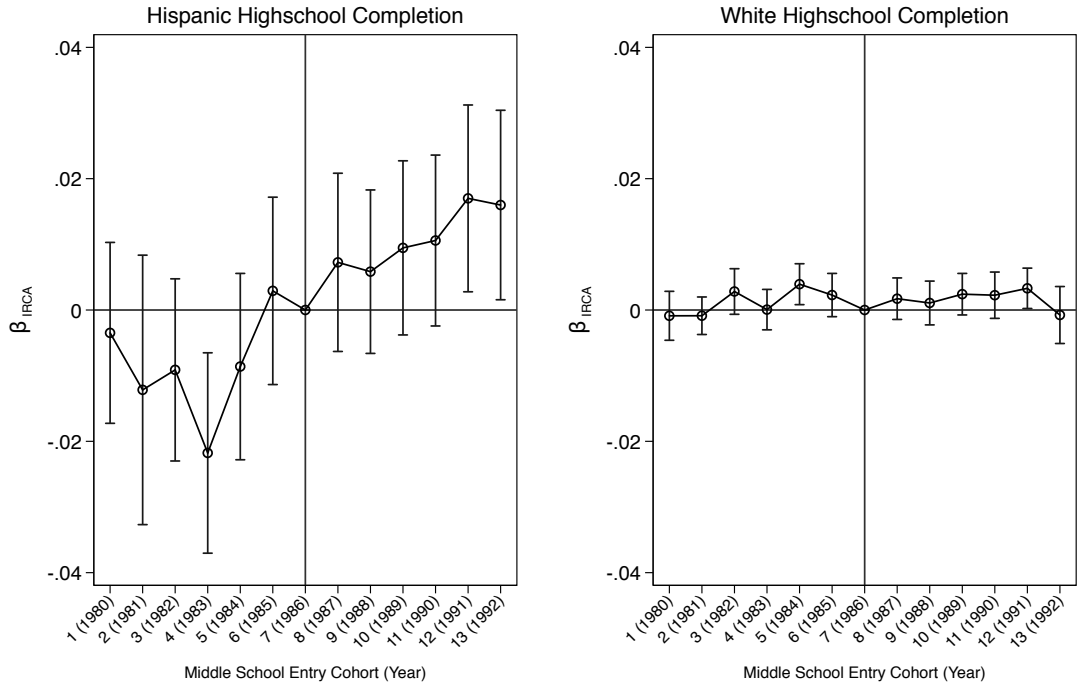

Figure 2.9: Event study estimates of high school completion on legalisation

Note: This graph plots the regression coefficient on legalisation intensity in 1992 when it is interacted with middle school entry cohort dummies as shown in equation 2.7. A person is placed in a middle school entry cohort depending on the year in which they were 12 years of age. The outcome variable, taken from the 2010 decennial census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school or more and zero otherwise. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. For the Hispanic sample, $N = 52,222$ whereas for the Caucasian sample $N = 133,907$.

2.8. Conclusion

Undocumented migration in the United States has become a deeply polarised issue. In this chapter, we set out to investigate the distributional consequences of giving undocumented migrants legal status through a nation-wide amnesty programme. Our contention has been that state governors allocate more resources to those counties where the newly documented migrants reside in an effort to win over their future political support. We substantiated this hypothesis in parts.

First, we found that documenting migrants does indeed have a significant distributional component. Across a number of specifications, our results consistently

demonstrate that as the share of documented migrants in a county increases, so too does the amount of per capita aid received by that county from its respective state government.

Second, in trying to understand why legal status affects the distribution of public finances, we uncovered political economy forces at work. We presented evidence that the allocation of state aid that arises out of the IRCA varies significantly according to the political context in which an incumbent governor finds him or herself. Governors transfer more resources to IRCA-affected counties when the governor is eligible for re-election, when the county is more politically contested, when the governor enjoys line-item veto power over the state budget and when the legislative and executive branches of state government are politically aligned. These results are especially noteworthy as it suggests that the relationship between legal status and the distribution of public resources is one of discretionary political choice rather than one of economic necessity or mechanical welfare increases.

In the final part of our analysis, we addressed the question of whether—and to what extent—state governors actually targeted resources to capture the political support of the newly documented migrants rather than that of other, perhaps competing, voting groups. In this respect, we exploited data from a key anti-migrant ballot measure as well as from survey data on attitudes and found little evidence of anti-migrant sentiment confounding our results. Lastly, we found that county expenditure in education increases significantly in IRCA-affected counties and that, consequently, Hispanic individuals, as compared to Caucasian ones, residing in those same counties experience significant improvements in educational outcomes, further suggesting that the resource allocation arising out of the IRCA is intended to service the needs, and win the political support, of the newly documented migrants.

On the whole, then, our findings point to a significant political economy dimension to immigrant legalisation. Offering legal status not only leads to various social and economic improvements at the local level but also provides politicians with strong electoral incentives to see that it does so.

Appendix B

B.1 Analysis of the Model

Our analysis begins by taking first order conditions of equation 2.1 with respect to g:

$$\frac{\partial \Pi}{\partial g}\bigg|_{g=g^*} = (1-\alpha)U_C'(g^*) + \alpha U_L'(g^*) + E \cdot \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \phi}{\partial g^*} - C'(g^*) \stackrel{!}{=} 0 \tag{8}$$

To understand how g^* responds to a sudden shock in legal status, α, we maximize 8 and this is implicitly given by the following:

$$\frac{\partial g^*}{\partial \alpha} = -\frac{U_L'(g^*) - U_C'(g^*) + E \cdot \frac{\partial \frac{\partial \Omega}{\partial \phi}\frac{\partial \phi}{\partial g}}{\partial \alpha}}{\frac{\partial^2 \Pi}{\partial g^2}\Big|_{g=g^*}} \tag{9}$$

Because $\frac{\partial^2 \Pi}{\partial g^2} < 0$ the sign in front of equation 9 becomes positive. Moreover, we have assumed that $U_L'(g) > U_C'(g) \ \forall \, g$; accordingly, the first term in the numerator, $U_L'(g^*) - U_C'(g^*) > 0$. The overall sign of equation 9 thus hinges on the sign of the second term in the numerator which can be expressed as follows:

$$\frac{\partial \frac{\partial \Omega}{\partial \phi}\frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \tag{10}$$

Rewriting $\frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial \phi} = \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha}$, and $\frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \cdot \frac{\partial g}{\partial g} = \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$ we can substitute these back into equation 10 to obtain:

$$\begin{aligned}
&= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\
&= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} \cdot \frac{\partial g}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\
&= \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \cdot \left(\frac{\partial^2 \Omega}{\partial \phi^2} + \frac{\partial \Omega}{\partial \phi}\right)
\end{aligned}$$

Under the assumption that $\frac{\partial \Omega}{\partial \phi} > |\frac{\partial^2 \Omega}{\partial \phi^2}|$, the overall sign of equation 10 is thus positive. This in turn allows us to state that $\frac{\partial g^*}{\partial \alpha} > 0$.[37]

[37] Although we have used a general functional form for Ω, for illustrative purposes, we set $\Omega(\phi) =$

Prediction 1: The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Given that the second term in the numerator in equation 9 is positive, we can state that $\frac{\partial g^*}{\partial \alpha}\Big|_{E=1} > \frac{\partial g^*}{\partial \alpha}\Big|_{E=0}$.

Prediction 2: The optimal allocation of state aid is larger when P is eligible for re-election and less when (s)he is a lame duck.

Finally, the functional form of $\Omega(\phi)$ leads us to a final testable prediction. Because $\phi = \phi_T$ represents an inflection point (where ϕ_T represents the winning threshold of an election), it follows that $\frac{\partial \Omega}{\partial \phi}$ is maximized as $\phi \to \phi_T$.

Prediction 3: The optimal allocation of state aid becomes larger the closer the P's expected vote share is to the winning threshold.

$\frac{1}{1+e^{-\phi}}$ and plot the various derivatives of $\Omega(\phi)$, shown in Figure B.1, to provide some intuition behind this assumption.

B.2 Additional Figures

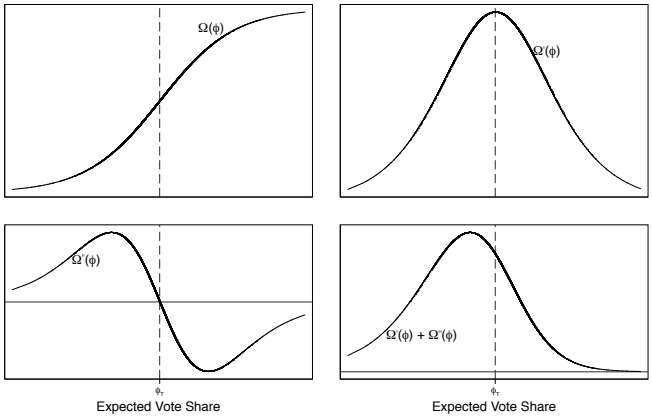

Figure B.1: Functional form of $\Omega(\phi)$, its first and second derivative and their sum

Note: This graph plots, clockwise from top-left: $\Omega(\phi)$; $\Omega'(\phi)$; $\Omega''(\phi)$; and $\Omega'(\phi) + \Omega''(\phi)$

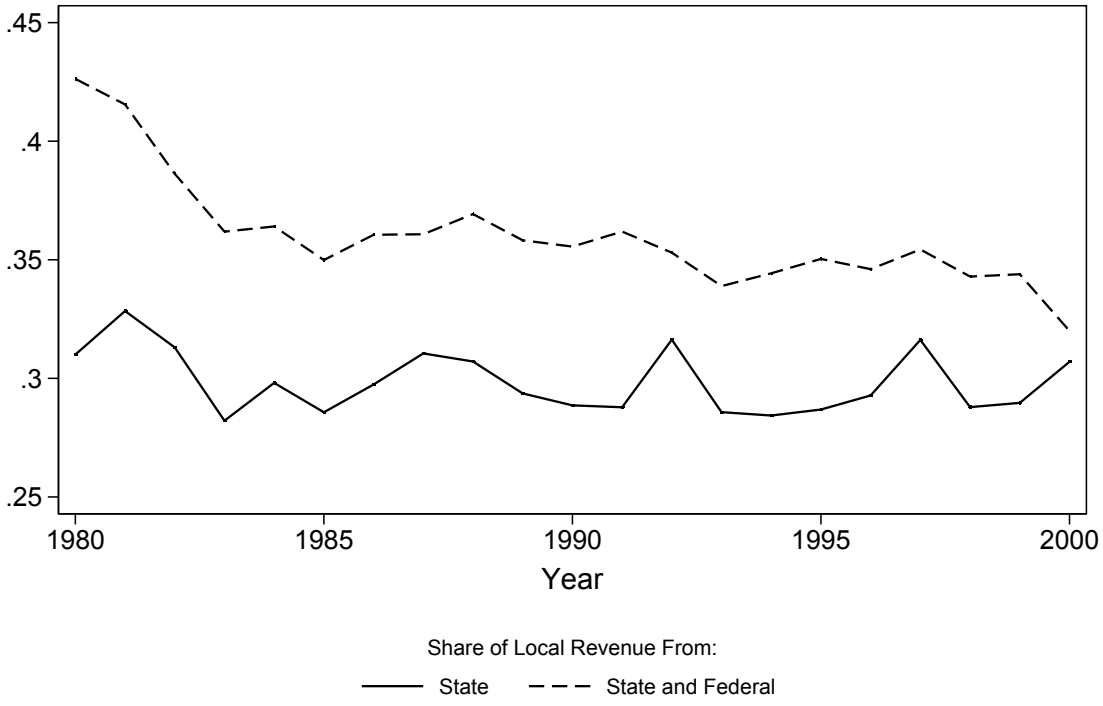

Figure B.2: Sources of local government revenue

Note: This graph plots the share of local government revenue (cities, municipalities and counties aggregated to the county) coming from state transfers and state and federal transfers.

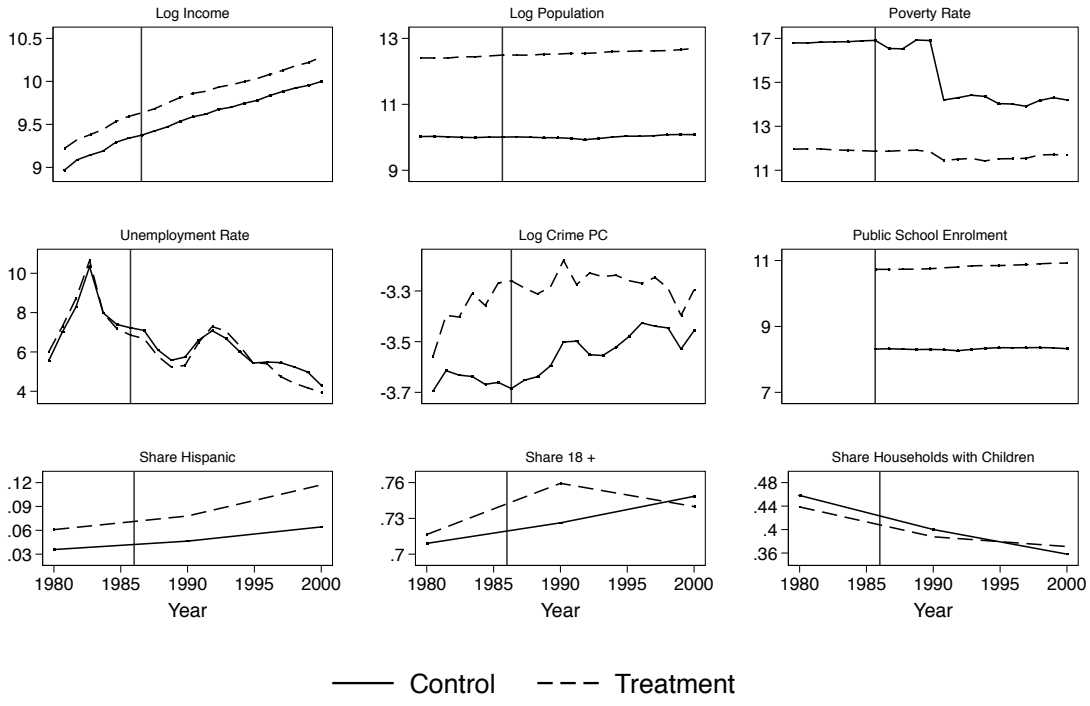

Figure B.3: Trends in county socio-economic characteristics

Note: This graph compares the evolution of various county characteristics in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.

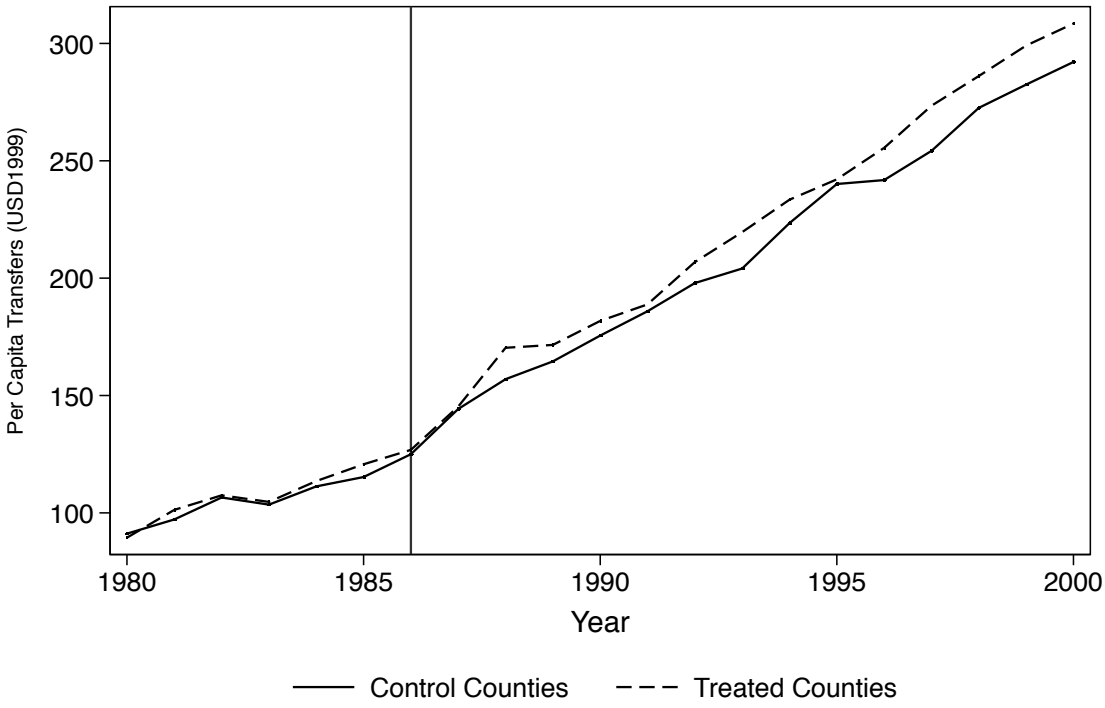

Figure B.4: Evolution of inter-governmental revenues in matched sample

Note: This graph compares per capita inter-governmental revenues (in USD1999) in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA in a sample of treated and control counties matched on the basis of propensity scores using the nearest neighbour. The county characteristics on which we base the propensity score matching are county income, population, crime, tax revenue, poverty rate and unemployment in 1980.

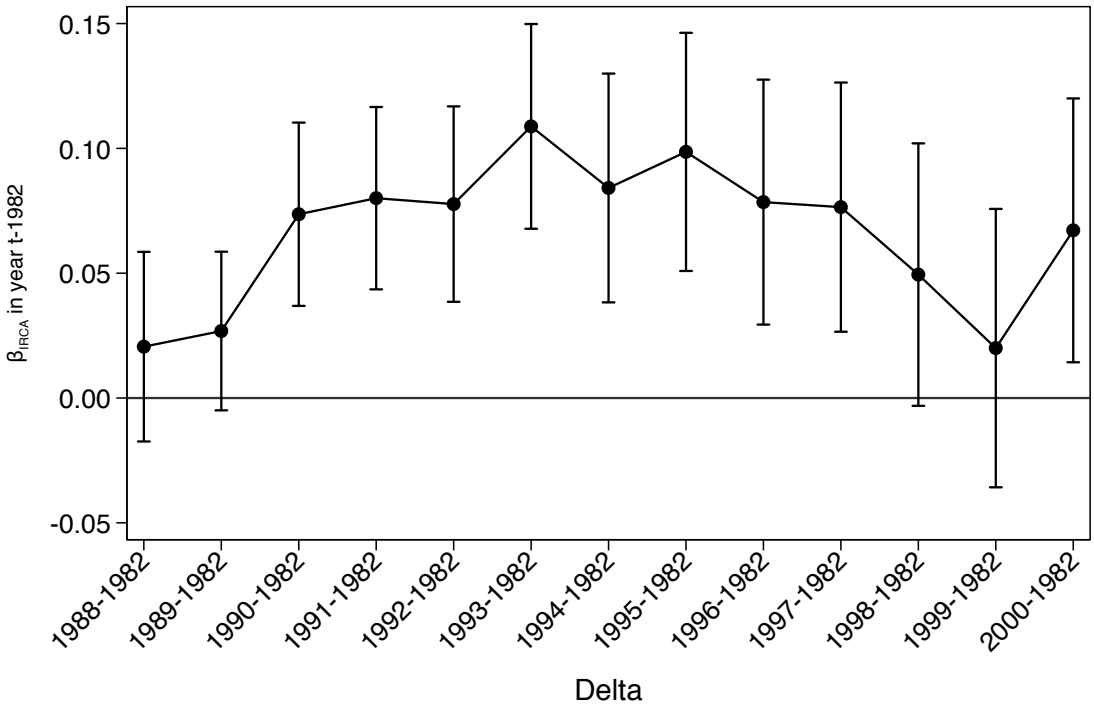

Figure B.5: First-difference coefficient estimates

Note: This graph plots the coefficients from various first-difference regressions from 1988 to 2000 using 1982 as the base year. The dependent variable is the log of per capita transfers from state to local governments (in 1999 USD) and β is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. County fixed effects and state-year fixed effects are also included in the estimations. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

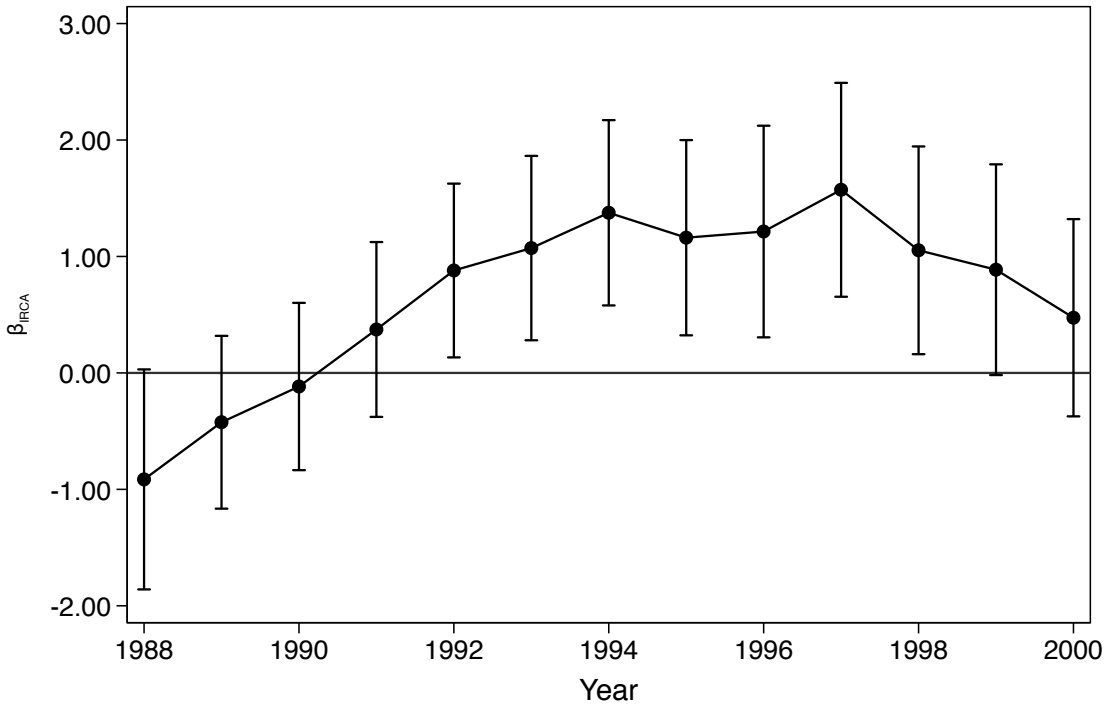

Figure B.6: Instrumental variables regression coefficients

Note: This graph plots β from 13 cross section regressions as specified in equation 2.5, one regression each for the years between 1988 and 2000. For each year, the value of the covariates is differenced from their 1982 value. The dependent variable is the log of per capita transfers from state to county governments (in 1999 USD) and β is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) when it is instrumented by the 1960 share of a county that is foreign-born. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

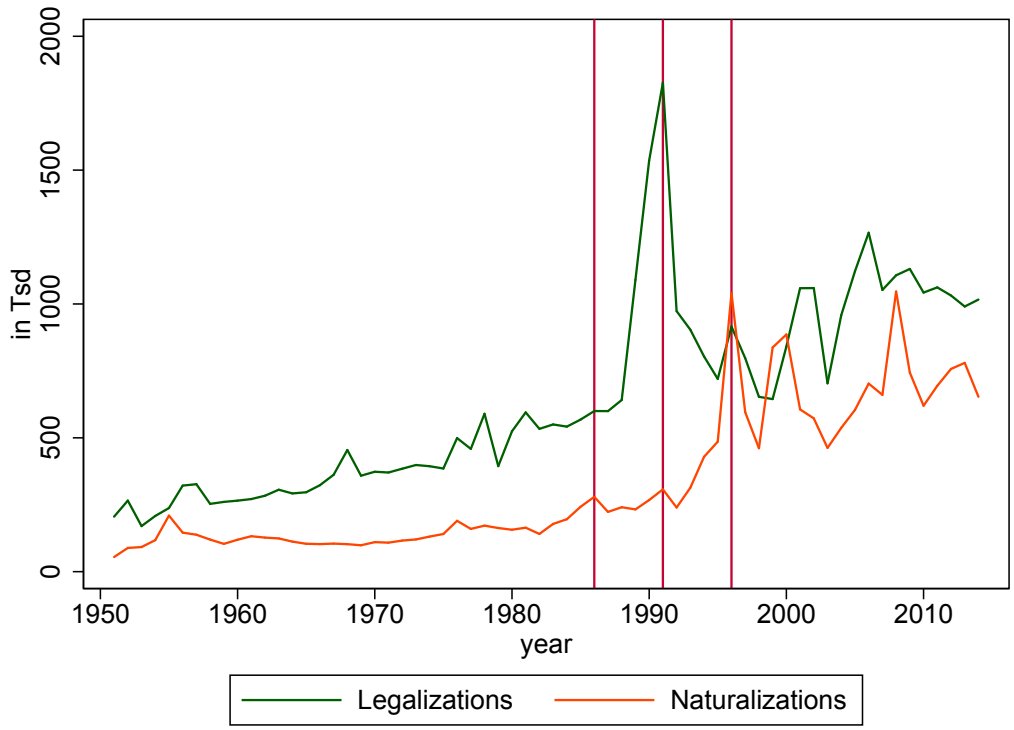

Figure B.7: Naturalisation and legalisation at the state level

Note: This graph plots trends in naturalisations and legalisations at the state level. Lines drawn at 1986 (the passage of the IRCA), 1992 (first cohort of naturalisations arising from the IRCA) and 1996.
Source: Immigration and Naturalisation Services, own data.

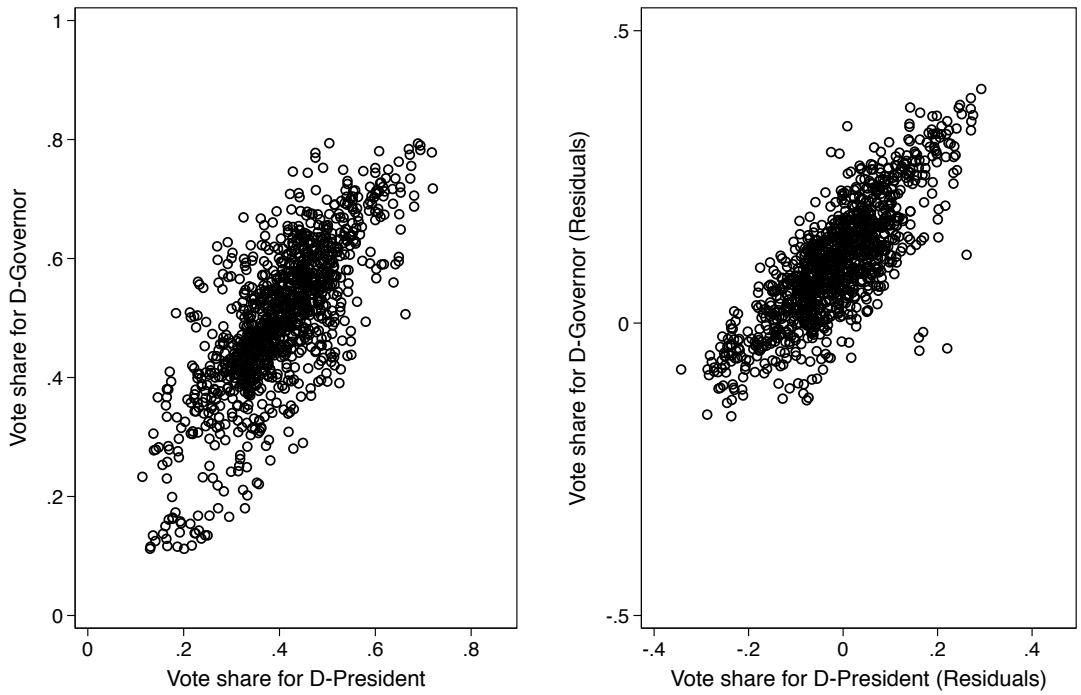

Figure B.8: Presidential and Gubernatorial election results

Note: These Figures plot the Democratic vote share at the county level in Presidential and Gubernatorial elections beginning in 1992. The scatter on the left plots the raw data while the scatter on the right plots the variables once state-year fixed effects and county fixed effects have been accounted for.

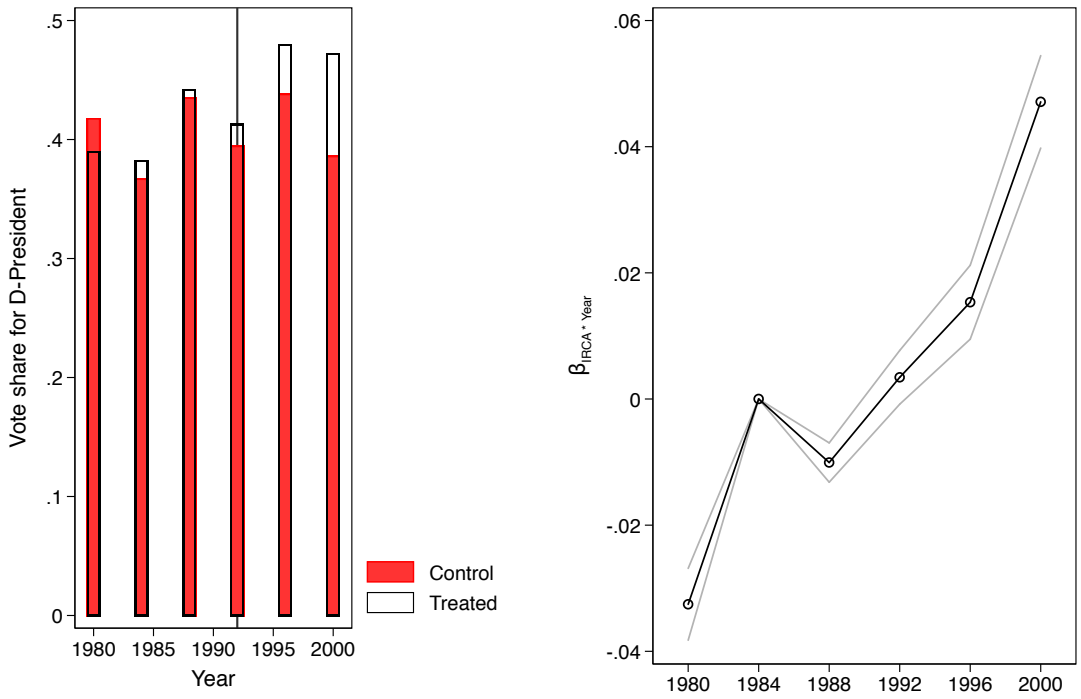

Figure B.9: The IRCA and the Democratic vote share

Note: The panel on left plots the Democratic vote share at the county level in Presidential elections in counties affected by the IRCA against those not affected by the IRCA. The panel on the right shows coefficients from a regression where Democratic vote share (in Presidential elections) is regressed on an interaction between a treatment indicator and year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 12,754$

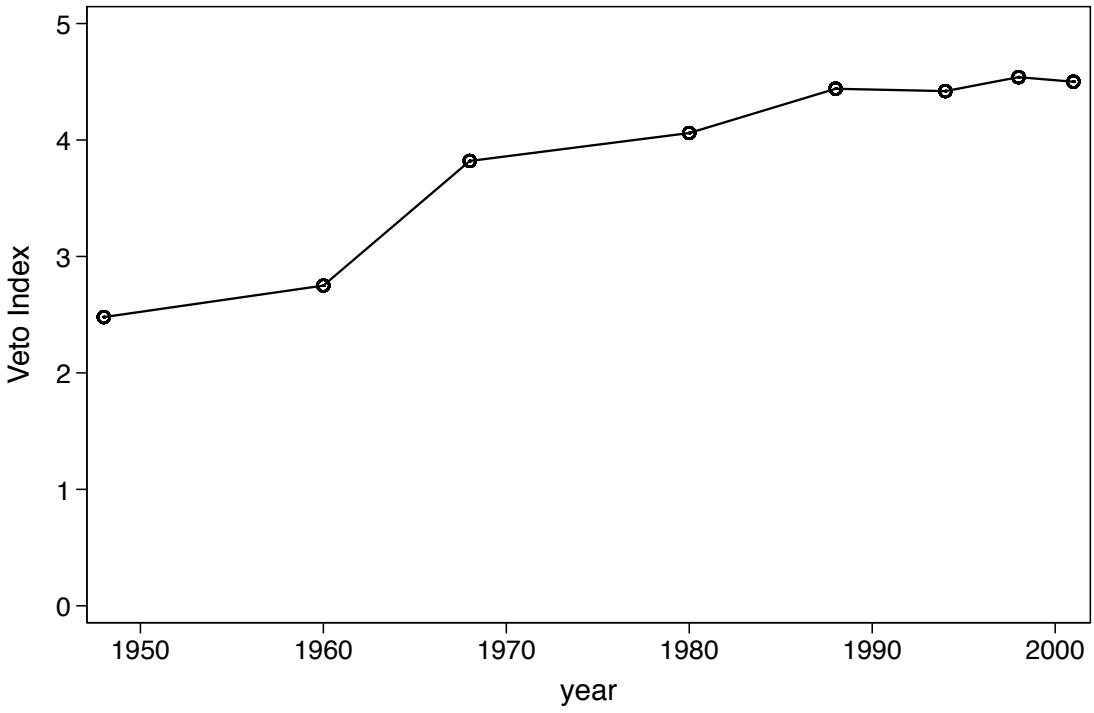

Figure B.10: Governor veto power index over time

Note: This graph plots an index of veto power enjoyed by state governors over time. The index is interpreted as follows: 5 Governor has item veto and a special majority vote of the legislature is needed to override a veto [three-fifths of the legislators elected or two-thirds of the legislators present]; 4.5 Governor has item veto, with a majority of legislators elected needed to override, except for appropriations bills when votes of two-thirds of those elected are needed to override; 4 Governor has item veto with a majority of legislators elected needed to override; 3 Governor has item veto with only a majority of legislators present needed to override; 2 Governor has no item veto, with a special legislative majority needed to override; 1 Governor has no item veto, with only a simple majority needed to override; 0 Governor has no veto of any kind.
Source: Thad L. Beyle

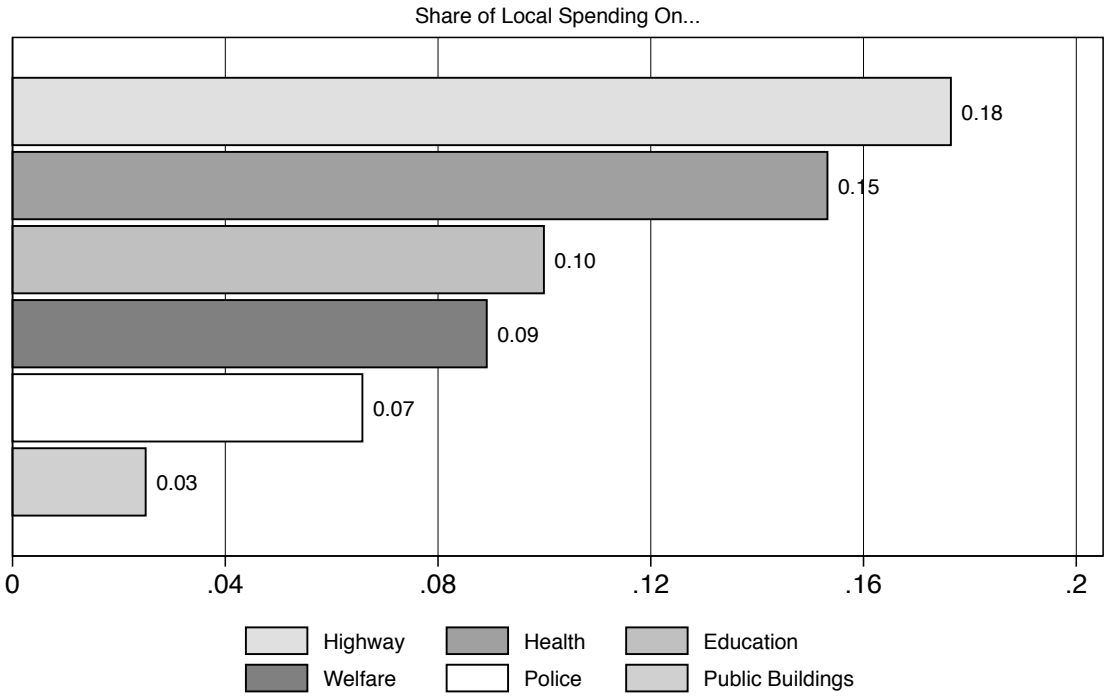

Figure B.11: Share of local expenditure on...

Note: This graph plots various categories of local government expenditure as a share of total local expenditure.

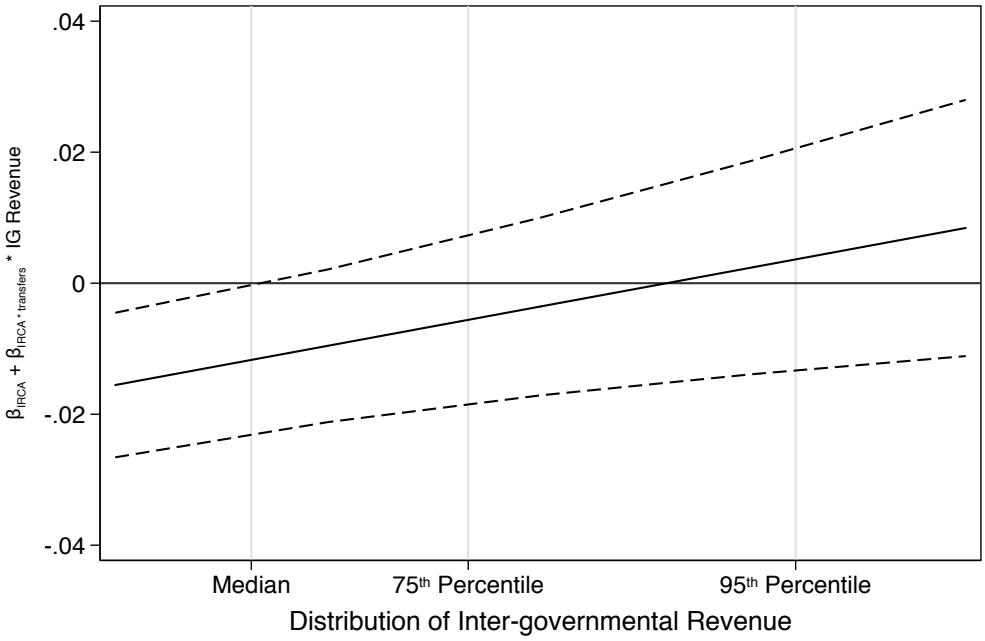

Figure B.12: Marginal effect of the IRCA on the propensity to vote

Note: This graph plots the marginal effect of immigrant legalisation on the propensity to vote when immigrant legalisation is interacted with inter-governmental revenue from the state. In other words, it plots $\frac{\partial \texttt{vote}}{\partial \texttt{IRCA}} = \beta_{\texttt{IRCA}} + \beta_{\texttt{IRCA}\times\texttt{transfers}} * \texttt{transfers}$ along the distribution of inter-governmental revenue. $\beta_{\texttt{IRCA}}$ is the coefficient on the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). The regression draws on individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. The outcome variable is an indicator that is one if an individual voted in that year's November election and zero otherwise. Control variables include individual race, sex, family income, marital status, education and age as well as year dummies and county population. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 41,968$.

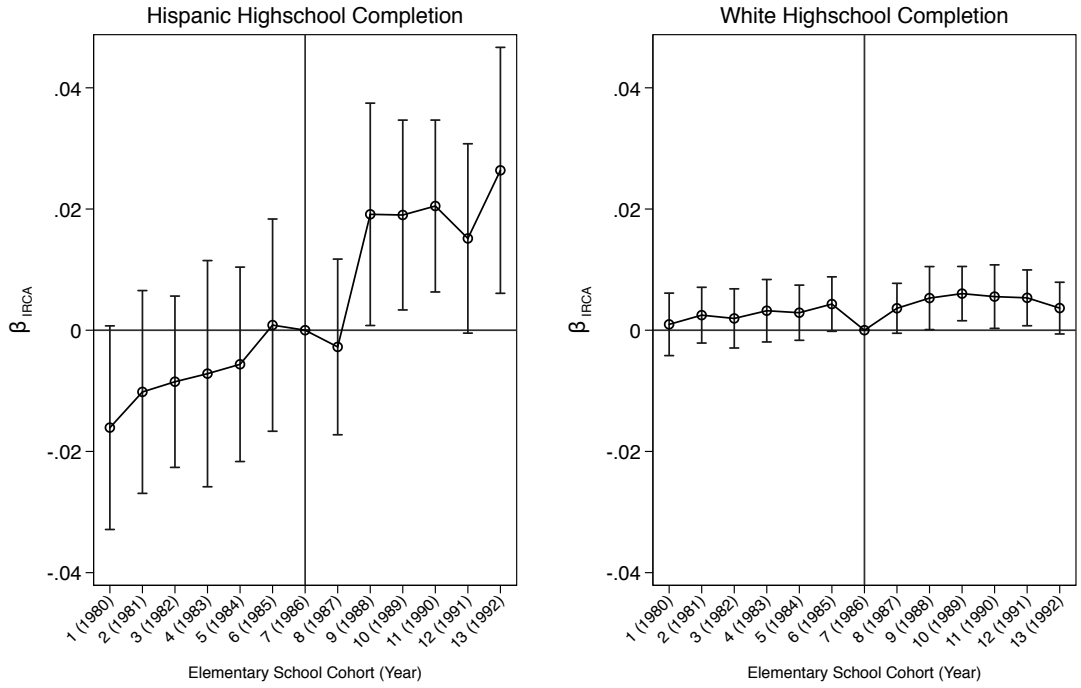

Figure B.13: Event study estimates of high school completion on legalisation

Note: This graph plots the regression coefficient on legalisation intensity in 1992 when it is interacted with elementary school entry cohort dummies. A person is placed in a elementary school entry cohort depending on the year in which they were six years of age. The outcome variable, taken from the 2010 decennial census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school and zero otherwise. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. For the Hispanic sample, $N = 53,580$ whereas for the Caucasian sample $N = 126,493$.

B.3 Additional Tables

Table B.1: Congressional Vote Record on the IRCA Bill

	House	Senate
Yes	274 (204-D; 70-R)	63 (34-D; 29-R)
No	132 (33-D; 99-R)	24 (5-D; 8-R)
Abstain	26	13

Notes: This table shows how the 99^{th} Congress voted for the IRCA Bill on 17 October 1986.
Source: Congressional Votes Database accessed at `govtrack.us`

Table B.2: Inter-governmental Revenue from State to Local Governments: Categories of Revenue

	Education	Health and Hospitals	Highways	Public Welfare
Includes	State aid for support of local schools; redistribution of federal aid for education; handicapped, special, and vocational education and rehabilitation; student transportation; equalization aid; school health; local community colleges; adult education; school buildings; and property tax relief related strictly to school funding.	State aid for local health programmes; maternal and child health; alcohol, drug abuse, and mental health; environmental health; nursing aid; hospital financing (including construction); and hospitalization of patients in local government hospitals.	State aid for construction, improvement, or maintenance of streets, highways, bridges, tunnels, etc.; distribution of state fuel taxes; and aid for debt service on local highway debt.	State aid for public welfare purposes; medical care and related administration under public assistance programmes (including Medicaid) even if received by a public hospital; care in nursing homes not associated with hospitals; federal categorical assistance (e.g., pass through of Aid to Families with Dependent Children, or AFDC); and administration of local welfare programmes.
Excludes	State grants for libraries; state expenditures on behalf of local schools for textbooks, buses, school buildings, etc.; and value of donated food commodities (non-revenue).	State aid for medical care under public assistance programmes such as Medicaid.	State grants for urban mass transit	

Notes: This table explains for what purposes inter-governmental revenue from state to local governments (counties, cities, municipalities aggregated to the county) is used for. We only observe these revenues in aggregate at the county level and do not observe the categories. This information is simply informative to give the reader an idea of the sorts of things a state governor can and cannot support with state-to-county transfers.

Source: Information taken from The Census Government Finance and Employment Classification Manual which can be accessed at: `https://www.census.gov/govs/www/classrevdef.html`

Table B.3: Baseline Results with Alternative Clustering and Inference

	(1) Treatment × Post	(2) Legalisation Intensity
$\widehat{\beta}$	0.0709	0.0610
p-values:		
A. Analytical values (clustered at the state level)	0.0325	0.0149
B. Wild Bootstrap values (clustered at the state level)	0.0521	0.0390
Observations	46,820	46,820
Number of States	46	46

Notes: This table presents the baseline estimates (Column 1 of Table 2.2) clustering the standard errors at the state level. p-values are derived both analytically, using Stata's conventional `vce(cluster state)` command as well as through Wild cluster bootstrapping generated using Roodman et al. (2018) `boottest` command.

Table B.4: Baseline Results using Linear Legalisations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop < 409,490	(4) Matching	(5) Linear Trends
Legalisations per 1,000 capita	0.00551***	0.0129***	0.00370***	0.00557***	0.00412***
	(0.00149)	(0.00332)	(0.00140)	(0.00164)	(0.00158)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
Linear Year Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

Notes: This table replicates the baseline estimation reported in Panel B of Table 2.2 but using the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants as the key independent variable without a log transformation. The dependent variable is the log of per capita transfers from state to local governments aggregated to the county in 1999 USD. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

State Legalisation Impact Assistance Grants (SLIAG)

Section 204 of the IRCA outlines the details associated with the State Legalisation Impact Assistance Grants (SLIAG)—a $1B per year federal funding programme for four years which could be spent over seven years until 1994. SLIAG was designed to compensate states for the extra costs they would incur as a result of the legalisation programme of the IRCA. Specifically, SLIAG funds were intended to assist states to defray expenses in the areas of public health, public assistance and education (Liu (1991); DHHS (December 1991)). It is unlikely that the SLIAG funds are confounding our results for the simple reason that SLIAG was administered through an entirely separate institutional set-up and is not part of the inter-governmental revenue outcome variable that we exploit.[38] Nevertheless, we obtain the amount of funding states received from SLIAG and deduct it from our main outcome variable to create an 'inter-governmental revenue net of SLIAG' variable. Results are shown in Table B.5 and confirm that the resource allocation to affected counties is not being confounded by SLIAG funds.

[38] As part of the IRCA, The federal government instituted the Single Point of Contact (SPOC) system, whereby every state designated its own SPOC so as to create "state-level lead implementation agencies to manage the [SLIAG] programme according to the unique needs and arrangements of the individuals states" Liu (1991). At the federal level, it was the Department of Health and Human Services that received applications for and disbursed the SLIAG funds but SLIAG required that SPOCs coordinate directly with state and local public health, public assistance and education organisations to receive the funds Liu (1991).

Table B.5: Transfers on IRCA Legalizations net of SLIAG Funds

	(1) Net Transfers PC
Log legalisations	0.398***
	(0.0807)
Control Variables	Yes
Year Fixed Effects	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	6,490
Number of Counties	2238

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD net of SLIAG funds received from the federal government. `Log legalisations` is the log of the cumulative number of IRCA legalised migrants in a given county in a given year per 1000 county inhabitants (plus one). SLIAG was made available until 1991 and so our net transfers variable is defined only until that period which explains the smaller number of county-year observations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Democratic Vote Share on IRCA Legalisations

	Democratic Vote Share		
	(1) All States	(2) D State	(3) R State
Log legalisations$_{t-5}$	0.0204***	0.0223***	0.0188***
	(0.00175)	(0.00333)	(0.00206)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	7,789	4,203	3,586
Number of Counties	2,141	1,127	1,014

Notes: The dependent variable is the Democratic vote share, at the county level, in Presidential elections. `Log legalisations`$_{t-5}$ is a five year lag of the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). In Columns 2 and 3 we split the sample and look at the relationship only in Democratic (Column 2) or Republican (Column 3) states defined by the party of the Governor in the year 1990. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Legalisation and Dynamics of the 99^{th} Congress

	Log of Inter-governmental Revenue (per capita)	
	(1)	(2)
Log legalisations	0.0442**	0.0444*
	(0.0221)	(0.0248)
Log legalisations × Majority D-Senators in State	0.0201	
	(0.0275)	
Log legalisations × Majority D-Members in State		0.0189
		(0.0293)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	46,820	46,820
Number of Counties	2,686	2,686

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). `Majority D-Senators in State` is an indicator that is 1 if both senators of a given state are Democrats and 0 otherwise. `Majority D-Members in State` is defined similarly: it is 1 if the majority of congress members from a given state were Democrats and 0 otherwise. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Legalisation and Term limits

	Log of IGR (per capita)
	(1)
	Incentive
Log legalisations	0.254
	(0.197)
Log legalisations × Electoral Incentive	0.0923***
	(0.0241)
Control Variables	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	12,134
Number of Counties	2,384

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. `Log legalisations` is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). `Electoral Incentive` is an indicator that is 1 if a governor is not a lame duck in the period between 1989 and 1994 and zero otherwise. The baseline effect of `Electoral Incentive` is captured by state-year fixed effects and is thus unable to be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: Local Spending and IRCA Legalisations

	Log of Per Capita Local Expenditure				
	(1) Total	(2) Health	(3) Education	(4) Welfare	(5) Highway
Log of Transfers	0.254***	0.285***	0.0435***	0.205***	0.245***
	(0.0184)	(0.0228)	(0.0113)	(0.0199)	(0.0193)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	36,869	36,869	36,869	36,869	36,869
Number of Counties	2,638	2,638	2,638	2,638	2,638
p-values:					
A. Unadjusted	9.8e-42	8.0e-35	.00011	2.0e-24	6.8e-36
B. Westfall & Young Adjusted	0	0	0	0	0

Notes: This table presents regression results using various categories of per capita local government expenditure as the outcome variable. `Log of Transfers` is the per capita inter-governmental revenue from the state to local governments aggregated to the county in 1999 USD. The adjusted *p*-values in the last row are adjusted to take into account potential multiple hypothesis testing using Westfall and Young (1993) family-wise error rate procedure. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: The IRCA, Citizenship and Voter Turnout using CPS

	(1) Naturalised	(2) Voted in Nov	(3) Voted in Nov
Log legalisations	0.0221***	-0.0060	-0.0770***
	(0.0046)	(0.0051)	(0.0207)
Log of Transfers			-0.0086
			(0.0085)
Log Legalisation × Log Transfers			0.0126***
			(0.0036)
Individual Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	50,206	46,210	41,968

Notes: This table presents regression results using individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. `Log legalisations` is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). `Log Transfers` is the per capita inter-governmental revenue from the state government to the county in 1999 USD. Column 1 has an outcome variable that is 1 if an individual is a naturalised citizen and 0 if (s)he is a native citizen. Columns 2 and 3 have as outcome variables indicators that are 1 if an individual voted in that year's November election and 0 if (s)he did not. The years for which we have data include three major gubernatorial election cycles as well as two Presidential elections but we cannot distinguish which election an individual voted for. Control variables include individual race, sex, family income, marital status, education and age. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

Legal Status and Political Representation: The 1986 IRCA and Hispanic Public Officials

3.1. Introduction

Recent midterm elections in the United States have been distinguished for producing the youngest and most diverse group of federal representatives to serve the country (Manning, 2018; Jin, 2019). Still, just 46 of the new members elected to Congress in the 2016 midterm elections were Hispanic, comprising approximately eight percent of that body even though Hispanic people make up about 18 percent of the United States population (Manning, 2018). For comparative purposes, the same midterm elections saw 52 African Americans assume public office while their share of the population stands at just over 13 percent (Jin, 2019).[1] Given that there are a number of studies that document the impact of a politician's identity and individual characteristics on his or her policy choices (Chattopadhyay and Duflo, 2004; Pande, 2003; Levitt, 1996; Besley, Montalvo and Reynal-Querol, 2011), an important question is what factors determine who gets elected to office.

Models of political selection suggest that candidates are selected to public office either on account of individual characteristics that make them particularly competent to implement policy (Caselli and Morelli, 2004; Besley et al., 2017; Besley and Reynal-Querol, 2011) or on account of their ability to represent a wider range of policy preferences (Osborne and Slivinski, 1996; Besley and Coate, 1997). Although some empirical work has shown that it is possible to for representative democracy to accommodate politicians of both high ability and broad representation (Dal Bó et al., 2017), citizen-candidate models in the spirit of Besley and Coate (1997) predict that the competence-representation dilemma is especially pronounced when capacity for

[1] Estimates of both the Hispanic and African American share in the population come from the Census Bureau Quick Facts Website: https://bit.ly/2GXW8JW. Accessed 7 March 2019.

policy-making is clustered among groups with specific policy preferences: in such cases, the electorate may very well sacrifice policy ability in favour of a candidate that better represents their interests.

In this chapter, I test whether documenting undocumented migrants—and eventually enfranchising them—affects the ethnic identity of those who assume public office and if so, whether it does on account of candidate competence or on account of better representation. To do so, I combine quasi-experimental variation in legal status arising from the 1986 Immigration Reform and Control Act (IRCA) with newly digitised data that contains information on over 43,000 Hispanic individuals elected to public office in the United States from 1984 to 1994. The IRCA legalised, and offered a path to citizenship, for some three million undocumented migrants, the vast majority of which are of Hispanic origin. The socio-economic characteristics of these migrants differed systematically to those of the native-born population. I therefore posit that these migrants are more likely to vote in Hispanic officials on account of their ability to better represent their interests, an effect which I expect to be the strongest in and around 1992 when IRCA documented migrants began to naturalise.

I utilise a difference-in-difference regression framework in order to estimate the gradient of legal status on the number of Hispanics in publicly elected office in a given county in a given year. The trends in the number of Hispanics elected to public office exhibit no distinguishable difference in treated and non-treated counties in the two periods prior to the passage of the IRCA in 1986, suggesting that geographical considerations with respect to the counties affected by the IRCA play little to no role in Hispanic selection to public office. This allows me to identify the difference-in-difference estimation which reveals a clear pattern: counties with larger shares of documented migrants experience positive and significant increases in the number of Hispanic persons holding public office from that same county, an effect that is amplified in counties with a larger pre-1986 stock of the population that is of Hispanic origin. Decomposing the data further reveals that the result is driven almost entirely by stronger representation at the local level, in particular school board officials and mayors. The point estimates are robust to alternative specifications and samples and to the inclusion of various socio-economic controls.

The timing of the effect reveals further patterns that suggest the result is driven by the voting potential of the newly documented migrants. By and large, but with notable exceptions, only citizens can vote in the United States.[2] Consistent with this fact,

[2]Legal permanent residents can, and have been able to in the past, vote in some local and municipal elections in the United States This was especially the case before the passage of the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 which subsequently restricted the right to vote in state and local elections to citizens. New York City, for example, allowed legal permanent res-

counties affected by the IRCA experience increases in the number of Hispanic elected officials at the state and federal levels but only as of 1993, one year after the first batch of IRCA documented migrants were eligible to vote owing to their naturalisation. Disaggregating local officers into county, municipal and school board levels reveals a clear positive effect for mayors of small cities beginning in 1992 and for school board officials in 1991, consistent with the fact that some states allowed legal permanent residents to vote in local school board elections. Given that the requirements to serve these local offices are more often concerned with ability to reflect the diversity of the community rather than they are with policy expertise, I take this as the first piece of evidence to suggest that representation, rather than pure competence, is driving the result.

To better understand to what extent Hispanic selection is driven by candidate competence, I decompose the data in order to examine the differential impact of the IRCA on Hispanic officials elected to state and local judgeships, a unique feature of the United States judiciary. Although the requirements to serve as judge vary, in general they are rather demanding and include, among other things, having a law degree and several years of professional experience as an attorney. These are characteristics that correlate strongly with measures of competence employed in the literature. If selection is driven by competence considerations alone, it is not unreasonable to expect an increase in the number of Hispanic judges in those areas most affected by the IRCA. To this end, I find no discernible effect for elected judges at any level. This finding, combined with the fact that the increase in elected officials is largest for local school board officials and mayors of small cities, suggests that representation rather than pure competence is the main driver of Hispanic selection to public office.

This study contributes to enhancing our understanding of the process of political selection. This scholarship began in earnest after Osborne and Slivinski (1996) and Besley and Coate (1997) articulated an alternative framework to that put forward by Downs (1957) for understanding policy credibility, convergence and candidate selection (Besley, 2005). This literature includes theoretical and empirical work that has helped us to better understand, among other things, the quality of public officials (Caselli and Morelli, 2004), what attracts high quality types to public office (Ferraz and Finan, 2009), the effects of high quality leaders on economic growth (Besley, Montalvo and Reynal-Querol, 2011) and to what extent it is possible to select leaders that are both of high quality and representative of a wide cross-section of society (Dal Bó

idents to vote in local school board elections from 1969 to 2002 and Chicago and a number of cities in Maryland allowed non-citizens to vote in a range of local elections. In the present day, three states allow legal permanent residents to vote in local and municipal elections and ten allow such residents to vote in special district and municipal elections.

et al., 2017) or of distinct groups (Besley et al., 2017). The question of politician identity has also received attention within the citizen-candidate framework. Pande (2003) and Chattopadhyay and Duflo (2004), for example, both consider the impact of political reservation for minority groups—scheduled castes and women, respectively—on policy outcomes in India. They find that stronger political representation of these groups leads to an allocation of public resources that positively affects those groups. In this chapter, I consider, in the absence of mandated political reservation, what factors enable minority groups to gain stronger political representation in the first place.

The paper also adds to the literature that aims at understanding the consequences franchise extension. The majority of these studies aim at testing models put forward by the likes of Downs (1957) and Meltzer and Richard (1981) and have advanced our understanding of the effects, on the level and distribution of public spending, of extending franchise to such groups as women Miller (2008); African Americans (Cascio and Washington, 2014); young people (Bertocchi et al., 2017); lesser-educated citizens (Fujiwara, 2015) and undocumented migrants (Sabet and Winter, 2019). It adds to these studies by considering not so much the economic consequence of franchise extension but its impact on the identity of those who run for public office.

Finally, this chapter utilises variation in legal status arising from the 1986 IRCA in order to understand the impact of legal status on political representation. Accordingly, the study also contributes to scholarship that documents the economic and social consequences of legal status. The majority of these studies examine the positive impacts of acquiring legal status on the social and economic outcomes of the individual migrant, including employment, wages and education (Kossoudji and Cobb-Clark, 2002; Pan, 2012; Amuedo-Dorantes and Bansak, 2011; Cortes, 2013) but also on more aggregate outcomes such as crime (Baker, 2015). To my knowledge, this is the first study to relate legal status with the question of political representation.

The rest of this chapter is organised as follows: in Section 3.2, I provide a brief account of the IRCA. To carry out this study, I digitised a novel source of data that contains information on Hispanic officials elected to public office from the local to the federal level. In Section 3.3, I describe this and other data used in the study. I outline my econometric methodology and present my results Sections 3.4 and 3.5 and I conclude in Section 3.6.

3.2. Background

3.2.1 The Immigration Reform and Control Act

As explained in greater detail in Sabet and Winter (2019), the 1986 Immigration Reform and Control Act (IRCA) is the most comprehensive piece of legislation the United States government has put forward in order to address the question of undocumented immigration. The purpose of the bill, which was signed into law in November 1986 by the Reagan Administration, was to restrict the inflow of undocumented migrants. It did so through three main mechanisms: an employer sanctions provision that made it illegal for employers to knowingly hire unauthorized workers, increased funding for border security to discourage new people from migrating illegally and an amnesty programme intended to legalise various unauthorized workers (Chishti and Kamasaki, 2014).

The amnesty provision of the IRCA is generally regarded as the most successful component of the law. It enabled undocumented migrants to apply for legal status from May 1987 until November 1988, depending on the circumstances of the person applying.[3] On acceptance of their application, applicants were given temporary legal status under the title of *Temporary Resident Aliens* which could last for as long as 18 months. During this time, their applications would be further processed and upon successful completion of an English test and a civics test, applicants were given permanent resident status. Five years after permanent residency, these migrants were eligible for naturalisation.

At the time of the Act, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Wasem, 2012; Baker, 2015) and the IRCA enjoyed near universal take-up (Baker, 2015). As detailed in Sabet and Winter (2019), the vast majority of these migrants were of Mexican origin, economically active and healthy. Although not wealthy, those documented by the IRCA earned, by and large, somewhere between the poverty rate and median income levels (Sabet and Winter, 2019). As to education: 40 percent of the IRCA applicants report having completed high school or more in 1989. The comparable figure for the population as a whole in 1990 stood at 75 percent.[4] Given that

[3]The IRCA provided paths to legalisation and naturalisation for two groups of unauthorized workers: Legally Authorized Workers (LAWs), which enabled undocumented immigrants who resided in the country for an uninterrupted period from before 1 January 1982 to legalise (DHHS (December 1991), Cascio and Lewis (2018)), and Special Agricultural Workers (SAWs) which allowed applications from unauthorized migrants who could show that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (DHHS (December 1991); Cascio and Lewis (2018)). LAW applicants were eligible to apply from May 1987 to May 1988 whereas SAW applicants from 1 June 1987 to 30 November 1988 (DHHS, December 1991).

[4]The educational attainment of the IRCA migrants comes from the Legalisation Population Survey

income and education are two measures of competence employed in the literature on political selection, these characteristics are useful to bear in mind when considering the question of whether Hispanic selection to public office is driven by candidate competence or his or her ability to better represent Hispanic interests.

The timing of the IRCA's passage in 1986 was unexpected. Legislation to address undocumented migration began in earnest after 1964 when the *Bracero* program—a guest worker program that the United States negotiated with Mexico in 1942—was terminated and since which time undocumented migration to the United States became an issue. Congress even attempted to sign comprehensive legislation a number of times but always failed (Chishti and Kamasaki, 2014). The prospects of the 1986 IRCA were also uncertain. Fuchs (1990), for example, reports that just days before the IRCA passed, "congressional leaders pronounced it dead, this time after more than fifteen months of hearings, legislative negotiations and debate". Reinforcing this notion, Representative Daniel E. Lungren (R-California) remarked on the day of the bill's passage that the IRCA was "a corpse going to the morgue, and on the way to the morgue a toe began to twitch and we started CPR again" (Fuchs, 1990).

The geographic distribution of IRCA documented migrants may pose a greater cause for concern. If, for example, undocumented migrants are concentrated in counties with larger Hispanic populations where the likelihood of a Hispanic running for office is greater, then the effect may reflect other, omitted factors rather than that of legal status on the ethnic identity of public officials. In this respect, the pre-trends are informative. Although the roughly 300 IRCA-affected counties differ with respect to certain observable characteristics to the rest of the counties in the country, as shown in Table 3.1, these two county types exhibit no discernible difference in *trends* with respect to the number of Hispanics in public office in the two years leading up to the passage of the IRCA in 1986. This is made clear in Figure 3.1 which plots the total number of Hispanic public officials in counties affected by the IRCA as compared to those unaffected by the IRCA, as it appears in the raw data.[5]

Public Use File which surveyed 6,193 of the migrants legalised by the IRCA. The educational figures for the United States population are from the 2000 Census Atlas of the United States. Chapter 10 of that Atlas reports on education and it can be found online: https://bit.ly/2jjoVBb. Accessed 7 March 2019.

[5]In the regressions, I use a log scale for this outcome. I therefore plot the same figure but using a log-scale in order to visualise the pre-trends. The result is shown Figure 1 and indicates little difference to the linear scale.

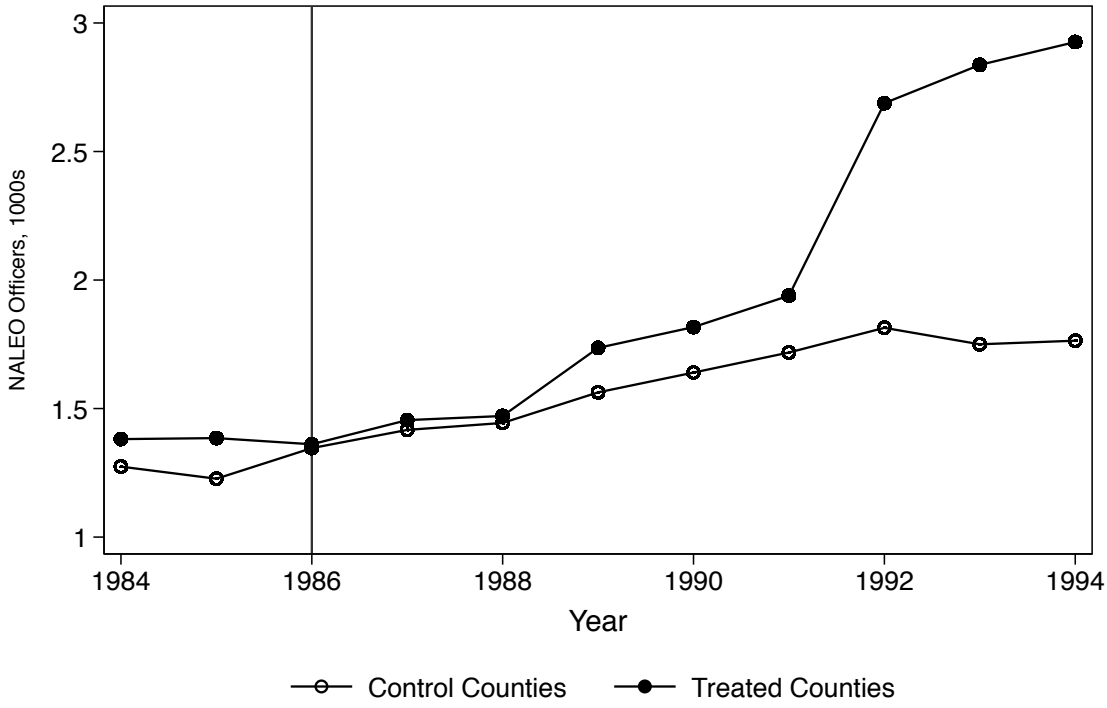

Figure 3.1: Number of Hispanics in elected public office by county type

Note: This graph plots the number of Hispanics in elected public office in counties affected by the IRCA as compared to those counties unaffected by the IRCA.
Source: NALEO's National Roster of Hispanic Elected Officials

Table 3.1: Balance Table: Treated v. Untreated Counties in 1984

	Treated			Untreated			Difference	
	Mean	S.D	Counties	Mean	S.D	Counties	Mean	S.D
NALEO Officers:								
Total Naleo Officers	4.21	[17.1]	328	0.67	[3.90]	1890	3.54***	(0.45)
Total Naleo Elected	0.38	[1.55]	328	0.11	[0.79]	1890	0.26***	(0.056)
Total Naleo Appointed	0.15	[1.44]	328	0.0011	[0.046]	1890	0.15***	(0.033)
County Characteristics:								
Unemployment Rate	7.98	[1.84]	328	7.98	[2.27]	1890	-0.00078	(0.13)
Poverty Rate	11.9	[5.85]	328	16.8	[7.48]	1890	-4.94***	(0.43)
Population (1000)	399.0	[617.1]	328	32.1	[30.0]	1890	366.8***	(14.3)
Log of County Income	9.53	[0.19]	328	9.29	[0.18]	1890	0.24***	(0.011)
Log of Total Crime, Per Capita	-3.36	[0.64]	328	-3.67	[0.73]	1890	0.31***	(0.043)

3.3. Data

3.3.1 Legalised Migrants

The independent variable of interest is a measure of the number of IRCA applicants per 1,000 county inhabitants in the United States for the period between 1984 and 1994. In the treated counties (i.e. those counties that received at least 1 application for legal status), this value ranged from as little as .04 to as many as 50 applications per 1,000 county inhabitants. These data come from Baker (2015) who, in turn, takes it from the Immigration and Naturalisation Service (INS). I also take from Baker (2015) measures of county poverty, unemployment, population and income which I use as control variables.

3.3.2 Hispanic Public Officials: The NALEO Roster

The goal of the analysis is to understand whether legalising, and eventually enfranchising, migrants of a particular group bears any influence on their representation in politics, as measured by the number of persons of that particular group elected to public office. To measure the impact of legal status on this outcome, I digitised a novel source of data taken from the historical archives of the National Association of Latino Elected and Appointed Officers (NALEO), a non-profit, non-partisan organisation which has, among other things, gathered data on Hispanic persons elected to public office from the local to the federal level since 1984. NALEO has been tracking this data in two waves: from 1984 to 1994 through the *National Roster of Hispanic Elected Officials* and, following a change in methodology, from 1996 to present day through the *National Directory of Latino Elected Officials*. The present study digitised the early archive records of the *Roster*. I generate a dataset of over 43,000 individual officials in this time period that contains information on the level of office served, the title of the role, the political affiliation of the official, his or her gender and, perhaps most importantly, their address. I use ZIP code information of each officer in order to generate county-level aggregates of the total number of Hispanic public officials in a given county in a given year. County officers are aggregated to the county in which they serve whereas federal, state and municipal officers are aggregated to the county to which their ZIP code corresponds. Thus, for example, State Representative for Arizona Carmen Cajero (1986, DEM) is coded under Pima County because that is the county in which her ZIP code falls. It is not the case that all state and federal representatives have ZIP codes that correspond to the counties where their respective state capitols are located. Carmen Cajero serves as a case in point. She serves as a state representative for Arizona but is based out of Pima County, not Maricopa

County where Phoenix, the state capitol, is located. The more than 600 counties across which the Hispanic officials are distributed contain, on average, 11 officials, some of which contain just one officer and some of which contain more than 100.

NALEO collects its information via survey. It begins with a mailed questionnaire to its current and past members which ranges from 2,500 individuals in 1984 to over 5,000 in 1994. In addition, NALEO identifies potential Hispanic office holders by surveying state directories, membership lists of national and state organisations, *Who's Who in American Politics* and Election Results Directories. In some years, NALEO retained a national press clipping service to identify news articles that report on Hispanics newly elected to public office. All newly identified persons, as well as those who did not respond to the initial questionnaire, were followed up through an extensive telephone verification process. Each year, NALEO undertakes over 5,000 phone calls to verify Hispanic officials who assume public office. In addition, NALEO has benefited from Census data. In 1987 and 1992, the Census Bureau prepared its *Popularly Elected Officials* reports. These reports identify elected officials across all levels of government in the United States and provide a breakdown by race. In these years, the Census Bureau provided NALEO with a breakdown of governments where Hispanics occupied public office and a handful of new officers were identified as a result. However, these officers were almost exclusively at the special district level and did not affect NALEO's estimates for other types of office. As such, the NALEO *Roster* is the most comprehensive source of information on Hispanics elected to public office.

Trends in Hispanic officials

Figure 3.2 shows the overall trend in the number of Hispanics entering public office over the period of the sample from 1984 to 1994. As shown, in the years before 1988, there is modest growth in the number of such officials: from some 3,100 in total across the United States to approximately 3,300 in 1988. Thereafter, however, there is an appreciable increase to some 5,400 in 1994.

Race and public office

Whilst the trend in Hispanic officials in office is increasing, especially after 1988, the relative number of Hispanics in office is still relatively few. The 1992 and 1987 editions of the *Popularly Elected Officials* reports prepared by the Census Bureau note that there were 85,006 governments at all levels in the United States comprising 513,200 officials in 1992 and 83,236 governments with 497,697 officials in 1987. Of these officials, 96.7 and 97.5 percent in 1992 and 1987, respectively, were white. In both cases, less than 1 percent of locally elected officials were of Hispanic origin.

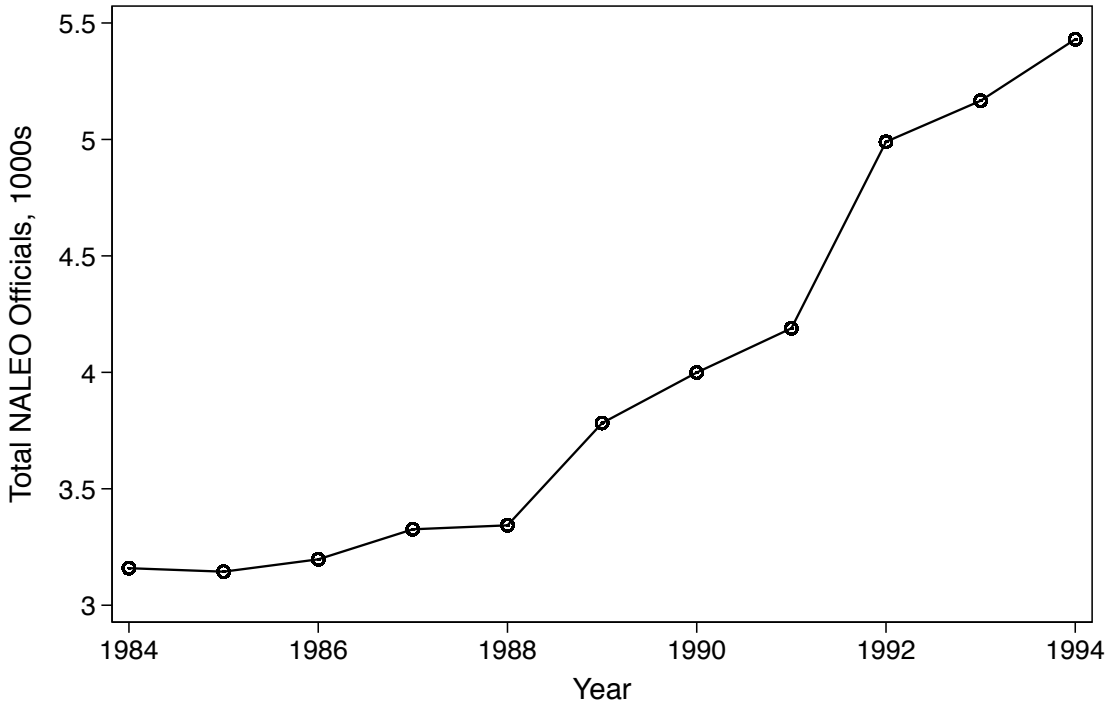

Figure 3.2: Total number of Hispanics in elected public office

Note: This graph shows the total number of Hispanics in elected public office at all levels of government across the entire United States.
Source: NALEO's National Roster of Hispanic Elected Officials

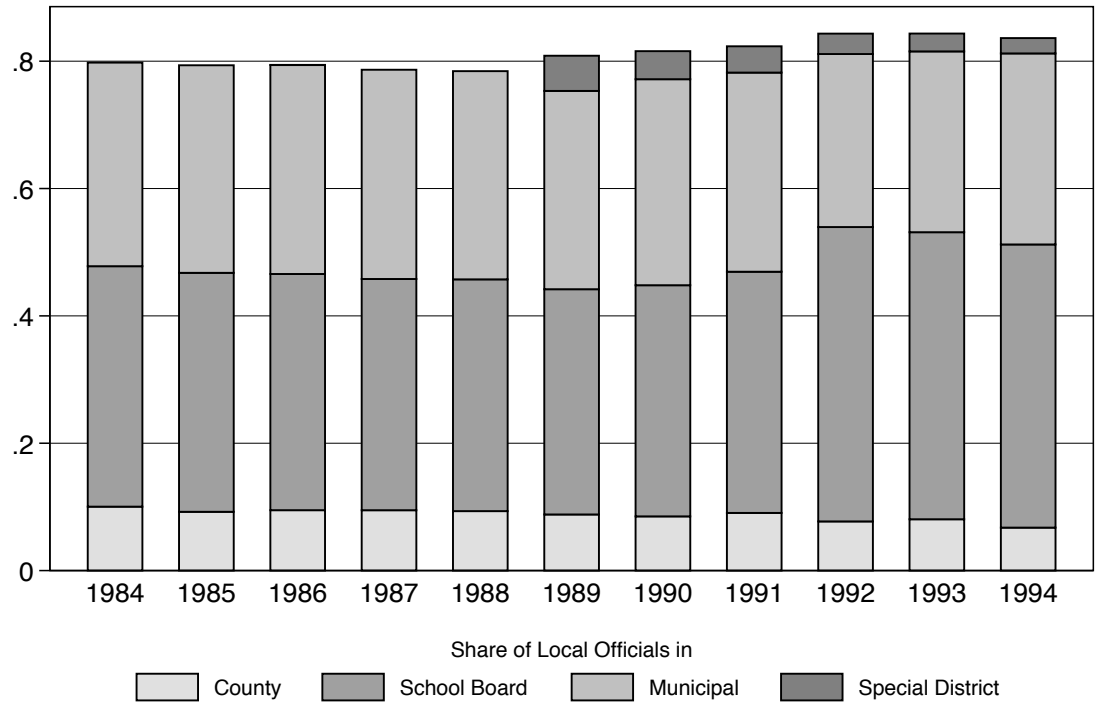

Figure 3.3: Hispanic elected officials at the local level

Note: This graph illustrates the breakdown among the 80 percent of Hispanic public officials that serve at the local level.
Source: NALEO's National Roster of Hispanic Elected Officials

Gender

The Census reports indicate that the majority of public officials are male: 76 and 80 percent in 1992 and 1987, respectively. The NALEO figures are comparable: on average, 79 percent of the NALEO public officials are men.

Levels of office

The Census reports indicate that the vast majority of elected officers—over 95 percent in both 1992 and 1987—serve local levels of government. The same is true of the officers contained in the NALEO *Rosters*. The data indicate that 80 percent of all Hispanic officials serve in local branches of government. Breaking this down further, as shown in Figure 3.3, reveals that the majority of these individuals serve as school board or municipal officials. This is perhaps unsurprising as these lower levels of office are typically considered as the first rung on the ladder of political power.

Political affiliation

The NALEO *Roster* contains information on political affiliation for 40,572 of the 43,000 officers. Of these, 56 percent are independent or non-partisan whilst 40 percent are registered Democrats. The remaining four percent are registered Republicans.

3.4. Immigrant Legalisation and Hispanic Officials

3.4.1 Testing for Pre-Trends

I begin the formal analysis by plotting the coefficient, β_j, of the event-study model specified in equation 3.1 in order to visualise the difference in trends between treated and non-treated counties with respect to the number of Hispanics in elected public office.

$$log(y+1)_{c,t} = \delta_c + \alpha_t + \sum_{j=1984}^{1994} \beta_j \left[T_c \times D_t^j\right] + \epsilon_{c,t} \tag{3.1}$$

Here, y denotes the number of Hispanics in elected public office in county c in year t. T_c is a binary variable set to one if a county received one or more applications for legal status post-1986 and zero otherwise; and D_t^j is a dummy set to one when $t = j$ ($\forall\, j \neq 1986$). I control for county-specific time-invariant characteristics through the inclusion of county fixed effects, δ_c, and I likewise account for any time-varying shocks by including year fixed effects, α_t. The error term is expressed as $\epsilon_{c,t}$ which I cluster at the county level. The results are shown in Figure 3.4 and indicate that the difference in the number of Hispanic elected officials is not significantly different for treated and non-treated counties prior to 1986, thus rendering the identifying assumption of the model plausible.

3.4.2 Baseline Estimates

Next, I control for county population, income, poverty and unemployment rates indicated by $\mathbf{X_{c,t}}$ in equations 3.2 and 3.3. In equation 3.2, I measure the difference between treated and non-treated counties by interacting T_c with an indicator, P_t, that is one if $t \geq 1986$ and zero otherwise. In equation 3.3, I estimate the impact of treatment intensity by exploiting the cumulative number of legalisations per 1,000 county inhabitants (plus one), as expressed by $ln(IRCA+1)_{c,t}$. In both estimations, I replace year fixed effects with state-by-year fixed effects, ζ_{st}, to capture any state specific, time-varying shocks that may confound the analysis.

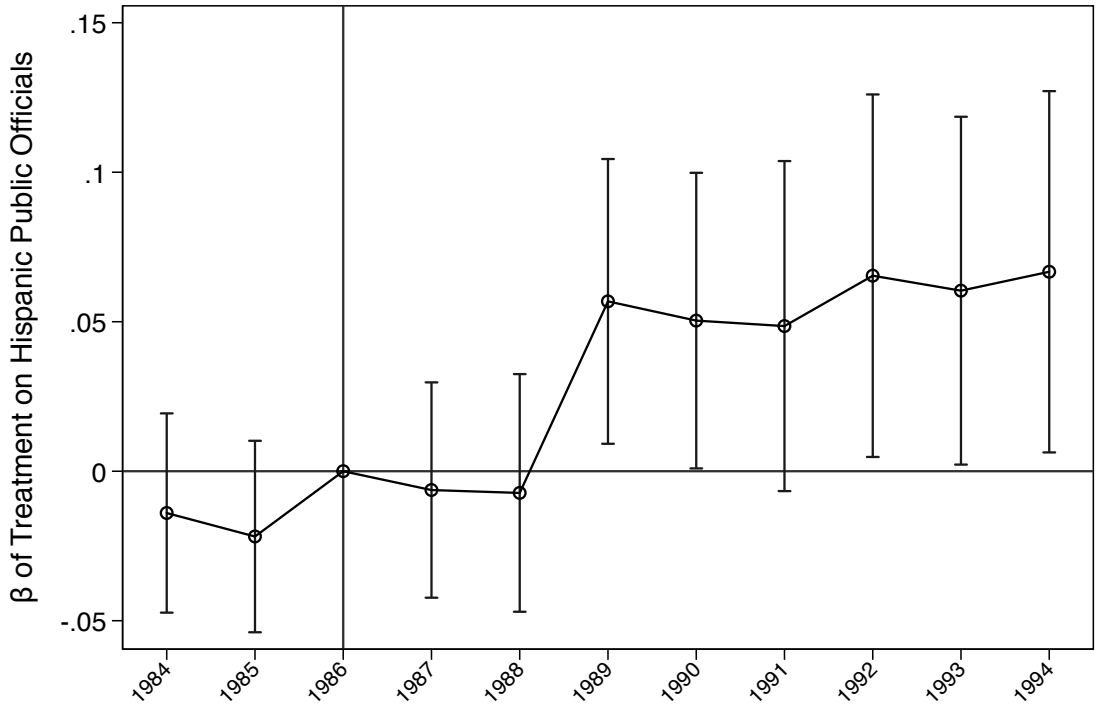

Figure 3.4: Coefficient on treatment for the number of Hispanics in public office

Note: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 3.1. The outcome is the number of Hispanics in elected public office in a given county in a given year. The regression includes no controls but includes county fixed effects and state-year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 24,358.

$$ln(y+1)_{c,t} = \beta_0 + \delta_c + \zeta_{st} + \beta_1 \cdot (T_c \times P_t) + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3.2}$$

$$ln(y+1)_{c,t} = \beta_0 + \delta_c + \zeta_{st} + \beta_1 \cdot ln(IRCA+1)_{c,t} + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3.3}$$

Results are shown in Table 3.2 and indicate a positive and statistically significant relationship. Panel A shows the estimated coefficient for β_1 when using a treatment indicator as shown in equation 3.2 whereas Panel B shows the same estimate when using treatment intensity as shown in equation 3.3. Column 1 shows the baseline effect: counties affected by the IRCA experience, on average, a 3.6 percent increase in the number of Hispanics in elected public office; measuring using treatment intensity, the result in Panel B indicates that a one percent increase in per capita legalisations at the county level is associated with a 0.058 percent increase in the number of Hispanics in office. Increasing the number of per capita legalisations from the 25^{th} to the 75^{th} percentile, this elasticity translates into an increase in the number of Hispanics in public office of around 26 percent.

In Column 2, I restrict the sample to those counties with populations less than the average population size (of the treated counties) in order to rule out the possibility that the effect is driven by a handful of large sanctuary cities. The results on this restricted sample are nearly the same as those of the baseline, indicating that very populous cities are not driving the effect. In Column 3, I return to the potential geographic endogeneity associated with where the IRCA migrants settle. I carry out a propensity score match in order to identify the most comparable control county for each treated county.[6] For this test, I drop the state-year fixed effects to allow for the possibility that the best-matched control county for a given treated county may, in fact, lie in a different state.[7] The results here are encouraging: not only are they precisely estimated, but they are also larger than before thus suggesting that, if anything, the baseline results may, in fact, be underestimating the size of the true relationship. Finally, in Column 4, I conduct a first differences estimation using only two time periods in the sample: 1984 and 1994. Skipping the intervening years allows me to (a) account for any issues that might arise with respect to IRCA application processing times and (b) compare one period before the passage of the IRCA with a period when many IRCA documented migrants were not only legalised but naturalised and, hence, enfranchised. Comparing treated and non-treated counties only in the first and last year of the sample only strengthens the results: it increases the magnitude of the coef-

[6] I match using the nearest neighbour when the propensity score is calculated on the basis of county population, income, poverty and unemployment rates as well as the per capita crime rate in 1984.

[7] Matching within a state and leaving state-year fixed effects in makes no difference to the result.

Table 3.2: NALEO Officials and the IRCA

	Outcome: Log Number of Hispanic Elected Officials + 1			
	(1) Baseline	(2) Pop $\leq 428,000$	(3) Matching	(4) $\Delta y_{1994\text{-}1984}$
Panel A. Treatment Indicator				
Treatment $\times$ Post	0.0362**	0.0356***	0.0787***	0.0637***
	(0.0142)	(0.0136)	(0.0227)	(0.0241)
Panel B. Treatment Intensity				
Log legalisations	0.0582***	0.0505***	0.0827***	0.0718***
	(0.0160)	(0.0162)	(0.0176)	(0.0221)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	No	Yes
Observations	25,699	24,690	7,056	4,162
Number of Counties	2,624	2,534	656	2,081

Notes: The dependent variable is the log of the number of Hispanic individuals in elected public office in a given county in a given year (plus one). Panel A shows results when using a treatment indicator whereas Panel B shows results when using a measure of treatment intensity which is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ficients and, in the case of Panel A, increases the precision with which it is estimated.

3.4.3 Robustness Checks

In Table 3.3, I undertake a number of empirical checks to test the strength of the relationship uncovered in the baseline estimations. These include lagging the explanatory variable by one or two years, as shown in Columns 1 and 2, and using the 1980 population as a basis for the per capita legalised calculation as shown in Column 3. The number of Hispanic officials elected to public office in a given county is likely to be influenced by the size of two key demographic populations: potential voters and those of Hispanic origin. To rule out mechanical increases in the number of Hispanic officials that might arise as a result of these factors, I control for the share of the county population that is (a) over 18 and (b) of Hispanic origin in Column 4. The results

remain robust to the inclusion of these characteristics.[8] In Column 5, I try to better understand the interaction between the Hispanic population of a given county and the IRCA. I therefore interact the number of per capita legalisations with the 1980 Hispanic population. The baseline effect of the 1980 Hispanic population cannot be estimated owing to county fixed effects. However, the rest of the interaction is informative: the baseline effect of the IRCA switches sign and loses its precision but its interaction with the Hispanic population is positive and significant, suggesting a dynamic interaction between the two: IRCA migrants have a stronger effect on the number of Hispanics elected to public office the larger the pre-existing Hispanic stock of the population, perhaps pointing to network effects at work.[9]

Finally, the reader may wonder to what extent the results are sensitive to the logarithmic transformation of the data, especially when so many counties have zero legalisations or Hispanic elected officials. To this end, I replicate the results of Table 3.2 but using a linear scale for both the dependent and independent variable. As shown in Table 1, the results are positive and precisely estimated. Like in Sabet and Winter (2019), I opt for a $log - log$ specification because both variables are unevenly distributed and because an elasticity is, in this case, easier for interpretation. Together, these results demonstrate that the relationship between legal status and the number of Hispanics assuming public office is a robust one.

[8] This is consistent with the fact that the IRCA did not lead to an increase in county population but instead a change in legal status of the already resident population. This is because population calculations are inclusive of the undocumented population. Undocumented migrants in the United States is an estimate based on the difference between the census population count, which includes everyone, and the legally resident foreign-born population which is kept by the Immigration and Naturalisation Services (INS) (Baker and Rytina, 2013).

[9] The results are also robust to more conservative approaches to inference. Clustering standard errors at the state level does not change the precision of the estimate (unreported).

Table 3.3: Robustness Checks

	Outcome: Log Number of Hispanic Public Officials				
	(1) $x-1$	(2) $x-2$	(3) 1980 PC	(4) Extra Controls	(5) Hispanic Population
Log legalisations$_{t-1}$	0.0571***				
	(0.0161)				
Log legalisations$_{t-2}$		0.0469***			
		(0.0168)			
Log legalisations				0.0495***	-0.128
				(0.0161)	(0.105)
IRCA per 1980 capita, Log			0.0532***		
			(0.0148)		
Log legalisations × Log of 1980 Hisp. Pop					0.0184*
					(0.0108)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	Yes	No
Observations	22,507	19,520	22,552	25,699	25,699
Number of Counties	2,561	2,491	2,213	2,624	2,624

Notes: The dependent variable is the log of the number of Hispanic individuals in elected public office in a given county in a given year (plus one). `Log legalisations` is the log of the cumulative number of IRCA legalised migrants in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.5. Heterogeneity Analysis

The richness of the NALEO data enables me to conduct a more refined analysis. In this section, I investigate whether there is a differential effect for officers serving at different levels of public office and I try to gain insight into the timing of the effect. I also try to understand to what extent Hispanic selection to public office is driven on account of a candidate's competence as opposed to his or her ability to better represent the interests of the Hispanic population.

3.5.1 Office Type

NALEO officers occupy a range of office types, from county and municipal officers, which include county executives, mayors and city councillors all the way to members of Congress.[10] To investigate whether there is a differential impact on the number of Hispanics entering a specific office type, I estimate β_1 from equation 3.4:

$$ln(y+1)_{o,c,t} = \beta_0 + \delta_c + \zeta_{st} + \beta_1 \cdot ln(IRCA+1)_{c,t} + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3.4}$$

Where the outcome variable is the log of the number of Hispanics in elected public office o in county c in year t (plus one) and all other variables are as previously defined. The results are shown in Figure 3.5. Whilst the overall effect is positive and significant, Figure 3.5 reveals interesting heterogeneities. The effects are largest for more localised and less politicised levels of office.[11] For example, the effect of legal status on the number of Hispanics entering office is the largest for school board officers (school board members, chairs, presidents, trustees) and municipal officers (mayors and city councillors) and whereas there is little to no effect for state level officers and a modest effect for federal members of congress.[12] This suggests that legal status has a significant impact on the ethnic composition of holders of public office but that this effect is most strongly felt at the local level.[13] To the extent that these higher offices correlate with more policy competence, as measured in the literature, these

[10]The only exception is that of State Governors. There are only two in the sample: Bob Martinez of Florida who served until 1990 and Toney Anaya of New Mexico who served until 1986. Because of the fewness of NALEO Governors, I am unable to estimate the effect of the IRCA on this particular type of office.

[11]I omit special district officials from these regressions because, as shown in Figure 3.3, NALEO did not collect data on these officials prior to 1986.

[12]I group all the federal, state and local officers, respectively, and regress these on the number of per capita legalisations in a county. The results are shown in Figure 2 and indicate that the effect is largest at the local levels.

[13]The judicial officers of Figure 3.5 include not just judges but also county sheriffs, town marshals, constables, police chiefs and other such officers.

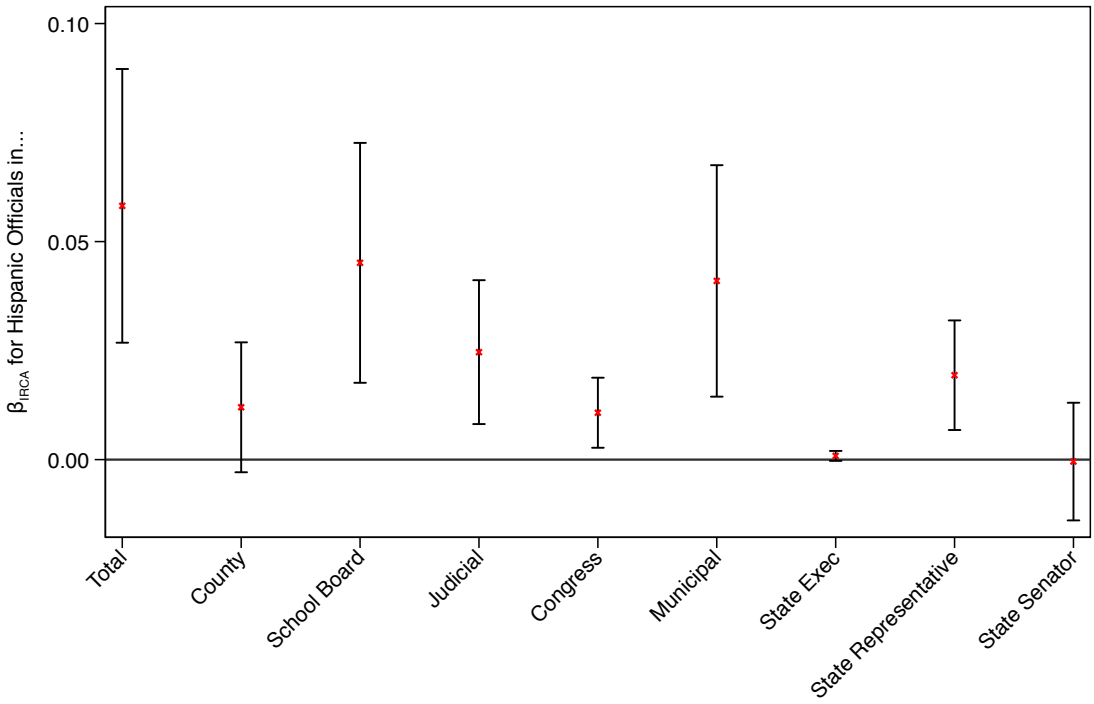

Figure 3.5: Regression coefficients: The IRCA and Hispanic public officials by office type

Note: This graph plots the regression coefficient on the log number of per capita legalisations (plus one) as specified in equation 3.4. The outcome variable is the log number of Hispanics serving a particular office type (denoted on the x-axis) in a given county in a given year (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. The regressions also include county fixed effects and state-year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 25,699 for all regressions.

results may also suggest that stronger representation in local matters is what drives Hispanic selection to public office.

3.5.2 Timing of the Effect

As a next step, I decompose the timing of the effect for elected officials to test the plausibility of the voting channel. Non-citizen voting in the United States has a long history. Up until the 1920s, 22 states allowed non-citizens to vote in local, state and even national elections (Raskin, 1992). In more recent times, the ability of non-citizens to vote in federal and state elections has been curtailed since 1996 and the enactment of the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) which criminalised non-citizen voting in such elections with penalties including fines, im-

prisonment and even deportation. However, the text of the law makes clear that "aliens are authorized to vote for such other purpose under a State constitution or statute or a local ordinance".[14] As such, there is some heterogeneity—even in present day—as to which states allow non-citizens to vote in local elections. At present, for example, a number of municipalities in Maryland, the cities of Chicago and San Francisco and the cities of Cambridge and Amherst in Massachusetts allow non-citizens to vote in local elections (Kini, 2005) while New York City allowed non-citizens to vote in local school board elections from 1968 to 2002 (Kini, 2005). The point is that while the majority of elections in the United States require citizenship, there are notable cases of non-citizen voting, especially at the local level. That the coefficient on legal status is largest for locally elected Hispanic public officials—a large number of which comprise school board officials—reflects this fact.

To better understand the timing of the effect, I plot β_j in Figure 3.6 after estimating it in the equation given by 3.5. Here, the outcome is the universe of Hispanic elected officials in county c in year t serving at level l where l represents officers at the local, state and federal levels.

$$log(y+1)_{l,c,t} = \delta_c + \zeta_{st} + \sum_{j=1984}^{1994} \beta_j \left[T_c \times D_t^j\right] + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3.5}$$

As shown in Figure 3.6, the coefficient on treatment for federal and state elected officials—that is, members of Congress, state representatives, state executives and state senators—is positive and precisely estimated only as of 1993 for federal officials and only in 1994 for state officials. This is consistent with the fact that documented migrants become relevant in such elections only after 1992 when they are eligible for citizenship and enfranchisement.

Interestingly, the coefficient on treatment for locally elected officials is not precisely estimated. I thus decompose these results further and estimate β_j when considering county, municipal and school board officials separately. The results are presented in Figure 3.7 and indicate the strongest effect for local school board members. Here, the pre-trends are flat and indicate virtually no difference in the number of Hispanics in school board positions in treated and non-treated counties prior to the IRCA and a sharp increase beginning in 1991, a year before eligibility for citizenship. Given that a number of states allow non-citizens to vote in such elections, the timing of the effect seems consistent with the idea that the result is driven by the voting potential of the IRCA documented migrants. Moreover, there are typically no professional or academic requirements to running for local school boards; instead, such positions

[14] 18 U.S.C §611 - Voting by Aliens.

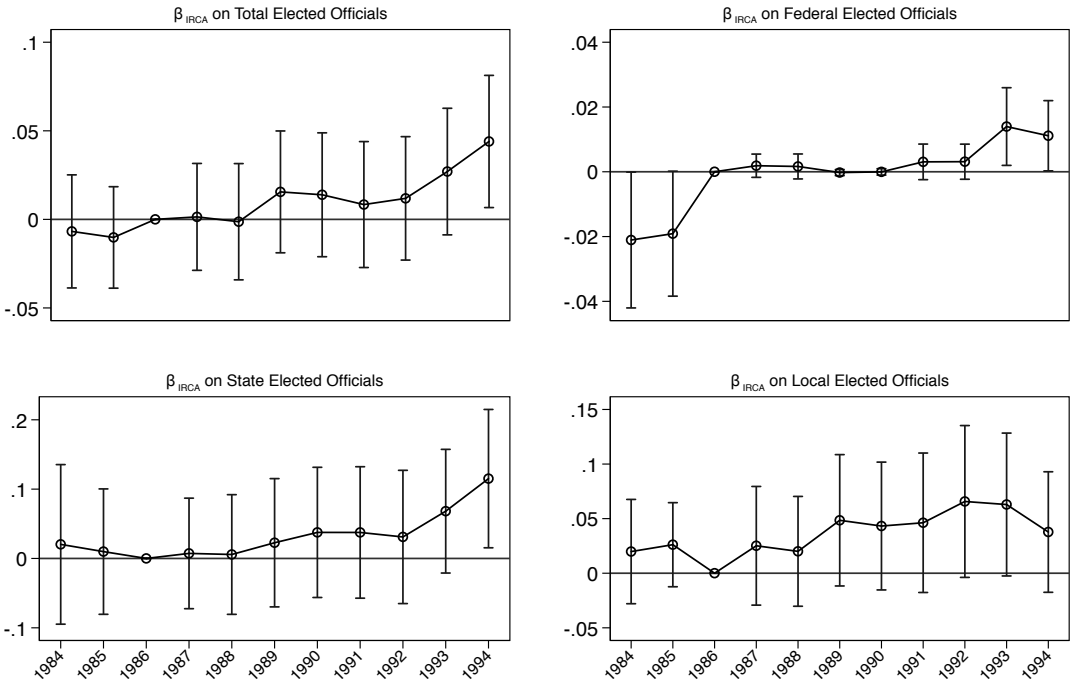

Figure 3.6: Regression coefficients: The IRCA and Hispanics in elected office over time

Note: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 3.5. The outcome variable is the log number of Hispanics serving in an elected role at different levels of government (excluding elected judgeships) in a given county in a given year (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. The regressions also include county fixed effects and state-year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 24,358 for all regressions.

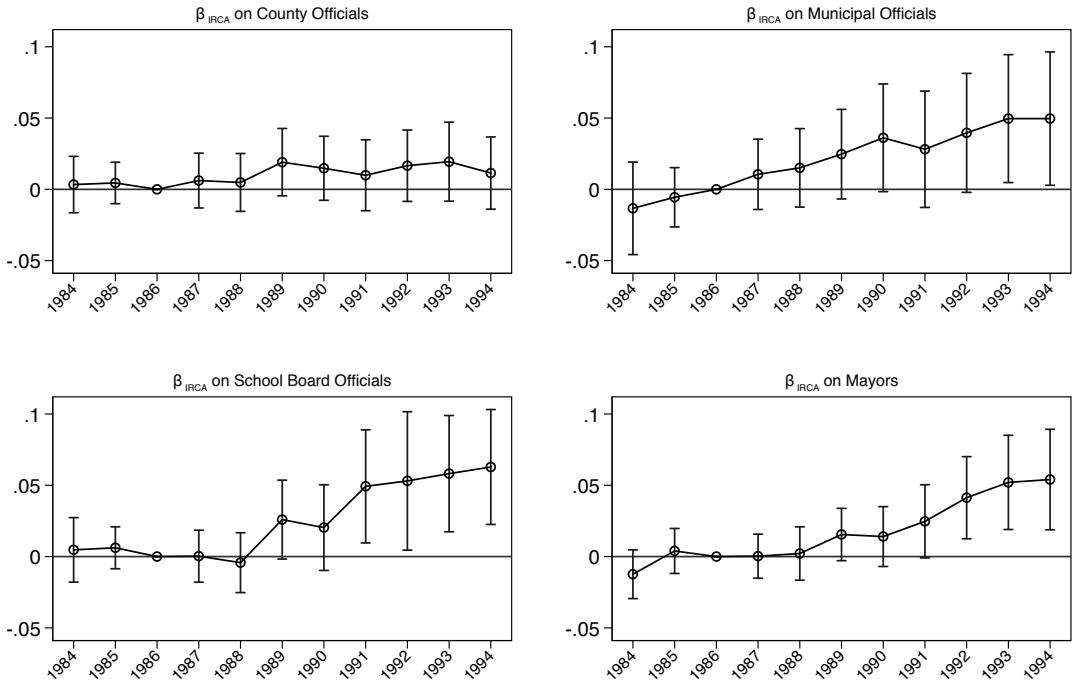

Figure 3.7: Regression coefficients: The IRCA and Hispanic local officials over time

Note: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 3.5 but only for those officers serving at the local level. The outcome variable is the log number of Hispanics serving in an elected role at the county, municipal or school board level (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. The regressions also include county fixed effects and state-year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 24,358 for all regressions.

often require candidates to be diverse and as representative of the community as possible. In this respect, the National School Boards Association of the United States explains that "[s]chool board members, especially those in large districts, are more representative of the communities they serve than state legislatures and members of Congress."[15] I take this as initial evidence that representation, rather than competence alone, is driving the result.

There is also a modest effect for municipal officials, a large number of whom are mayors. I therefore estimate the treatment effect over time separately for Hispanic mayors, as shown in the bottom-right panel of Figure 3.7 and find strong results. The coefficient on treatment both increases in size and becomes precisely estimated as of 1992, coinciding with the first year when IRCA migrants were eligible to vote in such elections. Because I lack data on the individual characteristics of the officials, I cannot test whether the Hispanic mayors who enter office in IRCA counties after 1986 differ in their socio-economic characteristics to those who assumed office before 1986. I therefore cannot say whether these mayors are chosen on account of their particular competence or their ability to better represent Hispanic interests. But, like school board officials, the formal requirements to run as a mayor do not involve certain professional or educational qualifications. Moreover, the Hispanic mayors in the data serve cities with populations of less than 300,000 where, it may be argued, an ability to represent the community may count for more than just policy expertise.[16]

3.5.3 Competence

In this subsection, I endeavour to understand what role competence plays in explaining the increase in Hispanic officials assuming public office as a result of the IRCA. A candidate's competence is usually proxied by measures of individual income, education, age and occupational status (Besley et al., 2017). Lacking this information my data, I am unable to test whether the Hispanic officials who enter office post-1986 in IRCA-affected counties differ systematically in their socio-economic characteristics to those who enter office prior to the IRCA. However, a peculiarity of the institutional context of the United States does allow me to carry out an indirect test for the competence channel: elected judges. Although qualifications do differ from state to state, most have relatively stringent requirements. In Texas, to take but one example, Judges from the county level to the state level are elected. At the county level, qualification for judicial office includes being a US citizen, at least 25 years of age, resident of that

[15] Taken from the Website of the NSBA: https://bit.ly/2xn7pyb. Accessed 7 March 2019.

[16] In fact, the effect vanishes when restricting the sample to counties with populations of more than 300,000.

Table 3.4: Elected Officials v. Elected Judges

	Outcome: Log Number of Hispanic Elected Officials	
	(1) Elected Officials	(2) Elected Judges
Log legalisations	0.0186**	0.00920
	(0.00798)	(0.00560)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	25,699	25,699
Number of Counties	2,624	2,624

Notes: The dependent variable is the log number of Hispanic individuals elected to public office in a given county in a given year (plus one). In Column 1 this includes the universe of all elected officials that are not judges. In Column 2 this includes only elected judges. `Log legalisations` is the log of the cumulative number of IRCA legalised migrants in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

particular county for at least two years and a licensed attorney who has practised law or served as a judge for four years.[17] Although some lower level judges have less exacting qualifications, candidates who run for elected judges are likely to correlate with higher measures of competence as used in the literature. Accordingly, in Column 1 of Table 3.4, I regress the legalisation variable against the total number of Hispanic elected officials from a given county in a given year, excluding elected judges. In Column 2, I carry out a similar exercise but use only elected judges from the NALEO *Roster* as my outcome variable of interest. The coefficient indicates that areas with higher numbers of documented migrants experience a greater number of elected officials but not elected judges. Given that the elected effect for non-judgeships is strongest for local school board officials and mayors of small cities and that there is no effect for elected judges, I conclude that Hispanic selection is being driven by factors other than just candidate competence.

To ensure that the coefficient in Column 2 is not hiding heterogeneities with respect to time or level of judgeship, I disaggregate elected judges at the state and local levels and plot the corresponding regression coefficients on a treatment indica-

[17]Taken from the Website of the Texas Judicial Branch: https://bit.ly/1T0JvRM. Accessed 6 March 2019.

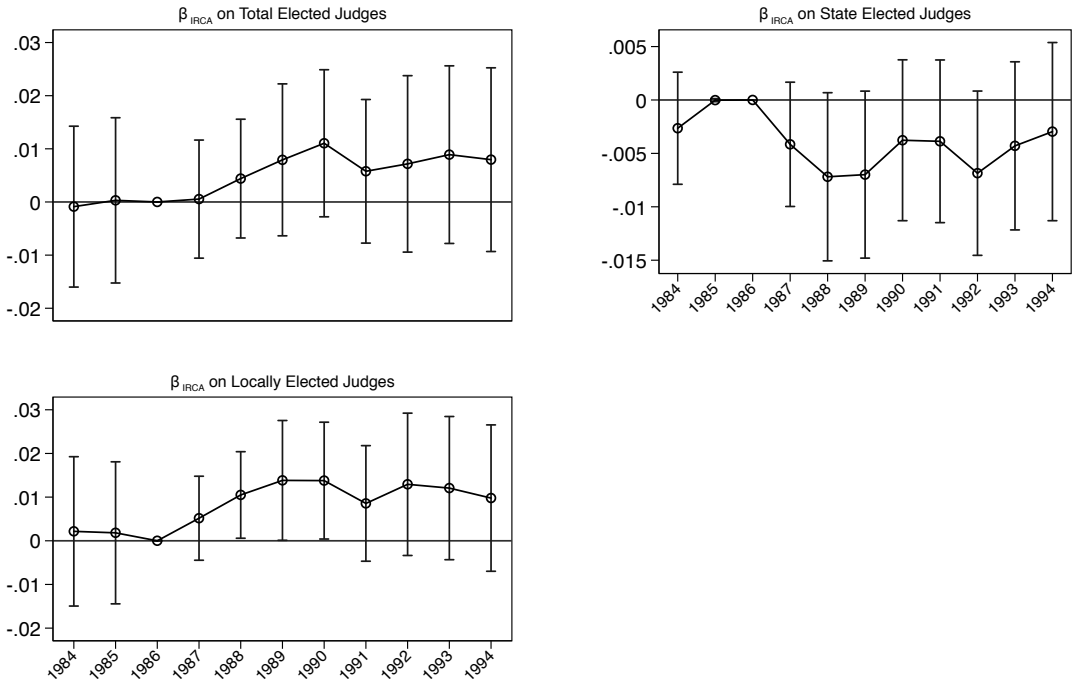

Figure 3.8: Regression coefficients: The IRCA and Hispanic elected judges over time

Note: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies. The outcome variable is the log number of Hispanic elected judges at different levels in a given county in a given year (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 24,358 for all three regressions.

tor when it is interacted with year dummies as shown in Figure 3.8. As illustrated, a county affected by the IRCA does not experience an appreciable increase in the number of Hispanic officials elected to judgeships after 1986, further reinforcing the notion that the Hispanics that assume public office as a result of the IRCA are selected for reasons other than just their competence.

3.6. Conclusion

Hispanics are the largest ethnic minority in the United States. Notwithstanding, Hispanic representation in politics is not reflective of their relative share in the population. Accordingly, this chapter has sought to better understand some of the underlying features of the institutional structure in the United States that determines who gets elected to office and on what grounds. The explanation I proposed is in line with models of political selection that highlight a candidate's ability to represent the interests of a particular group as a key driver of selection.

The particular institutional feature that I studied more carefully is that of Hispanic immigrant legalisation and enfranchisement. The idea is that a sudden change in the voting eligibility of a relatively homogeneous group of migrants of Hispanic origin increases their relative power in choosing their representatives. Exploiting a novel source of data that contains information on over 43,000 Hispanic public officials, I found that counties affected by the 1986 IRCA, in comparison with those unaffected by it, experience a significant increase in the number of Hispanics elected to public office after the passage of the law. The result is not confounded by geographical considerations with respect to where the undocumented migrants settle and I showed that the relationship remains robust to a number of empirical tests. I found, moreover, that the baseline effect is amplified the larger the stock of the pre-1986 population that is of Hispanic origin, suggesting strong network effects between new migrants and existing ones when it comes to selecting public officials.

The data on Hispanic officials allowed me to uncover interesting heterogeneities. In this respect, I found that counties affected by the IRCA experience greater numbers of Hispanic officials assuming office at all levels of government, but that the effect is strongest for locally elected public officials, in particular school board members and mayors. Examining the timing of the effect, I found that the number of Hispanics in office at the federal and state level increases significantly as of 1993 and at the local level as of 1991, broadly consistent with the time when IRCA documented migrants gained eligibility to vote via naturalisation, suggesting that the effect runs through the voting potential of the newly documented migrants. Moreover, given that the effect is strongest for local, less politicised offices that do not have professional or ed-

ucational prerequisites, I took this as initial evidence that representation, rather than competence alone, is driving the result.

In the final part of the paper, I examined to what extent Hispanic political selection in this setting is driven by competence. In this connection, I exploited the institutional feature of the United States that allows for many judges at the state and local level to be elected. The intuition is that if competence is a very strong driver of political selection, counties affected by the IRCA ought to experience an increase in the number of Hispanic judges elected to office as the requirements of that office correlate strongly with measures of competence typically employed in the literature. My results, however, indicate no significant relationship between counties affected by the IRCA and the number of Hispanic officials serving as elected judges. This finding, combined with the fact that the effect is the largest for local school board officials and mayors of small cities, leads me to conclude that the Hispanics that are selected to public office as a result of the IRCA are selected not strictly on account of their educational qualifications and professional credentials but rather for their ability to better represent Hispanic interests on local matters pertaining to schooling and education.

Appendix C

C.1 Additional Figures

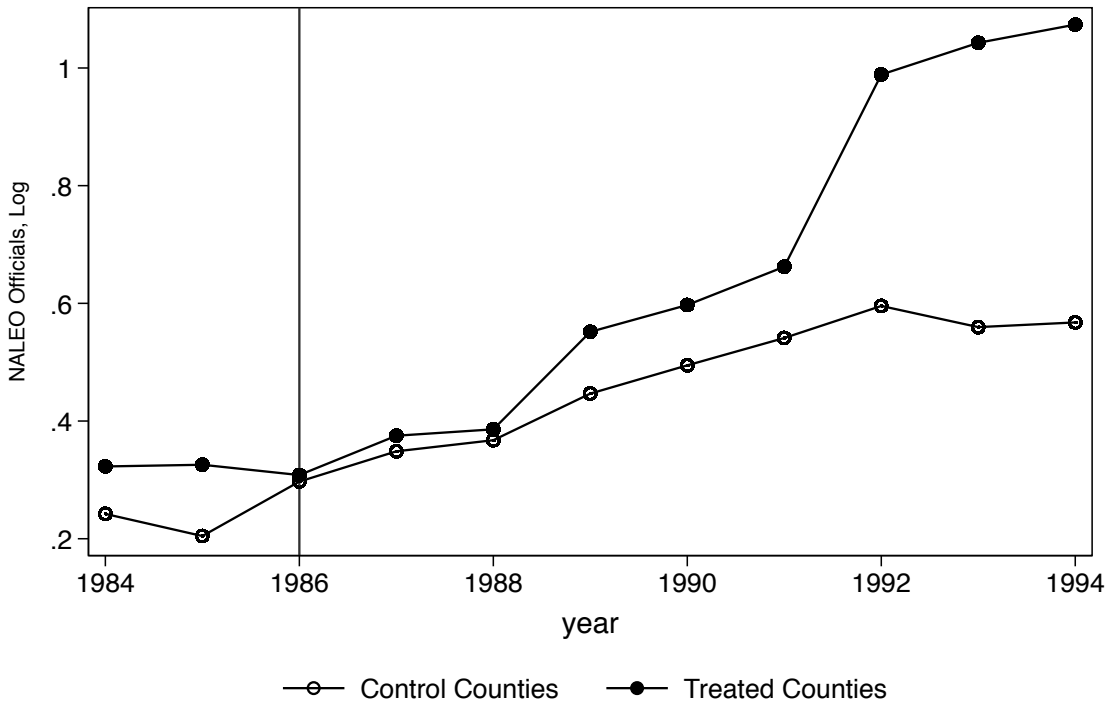

Figure 1: Number of Hispanics in elected public office (log scale) by county type

Note: This graph plots the log of the number of Hispanics in elected public office in counties affected by the IRCA as compared to those counties unaffected by the IRCA.

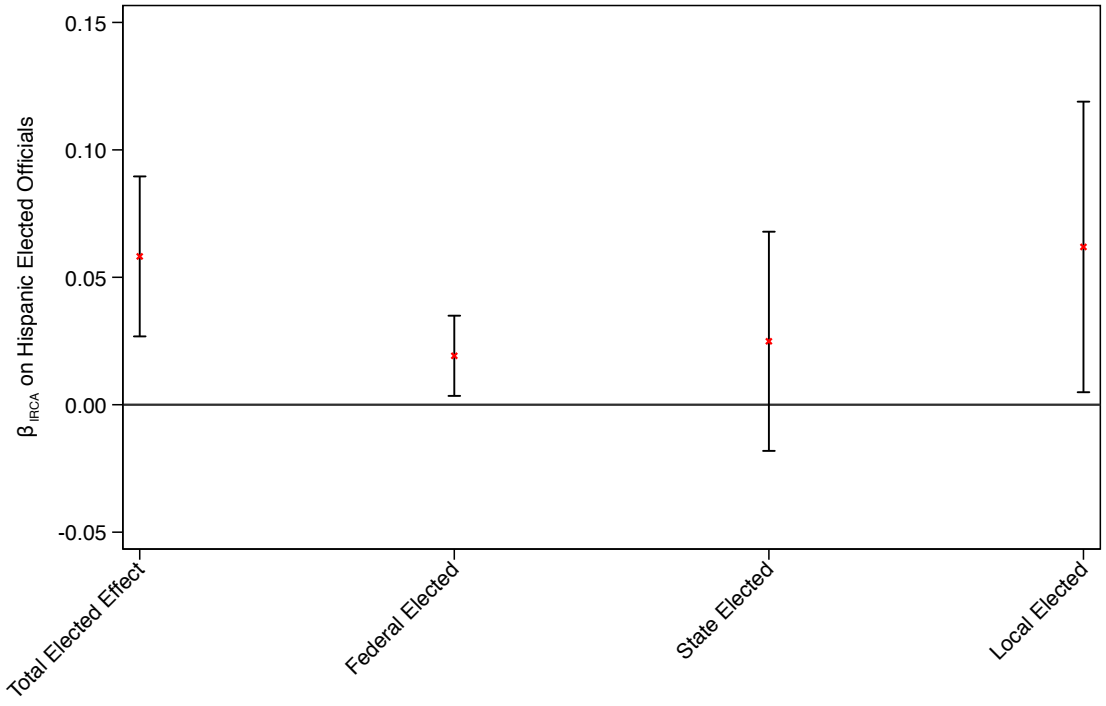

Figure 2: Regression coefficients: The IRCA and Hispanic public officials by levels of office

Note: This graph plots the regression coefficient on the log number of per capita legalisations (plus one) when the outcome variable is the log number of Hispanics in elected public office at the federal, state and local levels as well as on the total number of Hispanic elected officials at all levels (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. The regressions also include county fixed effects and state-year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent. N = 25,699 for all regressions.

C.2 Additional Tables

Table 1: NALEO Officials and the IRCA using a Linear Scale

	Outcome: Number of Hispanic Elected Officials			
	(1) Baseline	(2) Pop $\leq 428,000$	(3) Matching	(4) $\Delta y_{1994\text{-}1984}$
Panel A. Treatment Indicator				
Treatment $\times$ Post	1.157**	0.388***	2.194*	2.192***
	(0.457)	(0.0982)	(1.172)	(0.730)
Panel B. Treatment Intensity				
Legalisations per 1000 capita	0.342**	0.104***	0.356**	0.587**
	(0.151)	(0.0279)	(0.175)	(0.251)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	No	Yes
Observations	25,699	24,690	7,056	4,162
Number of Counties	2,624	2,534	656	2,081

Notes: The dependent variable is number of Hispanic individuals in elected public office in a given county in a given year. Panel A shows results when using a treatment indicator whereas Panel B shows results when using a measure of treatment intensity which is the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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